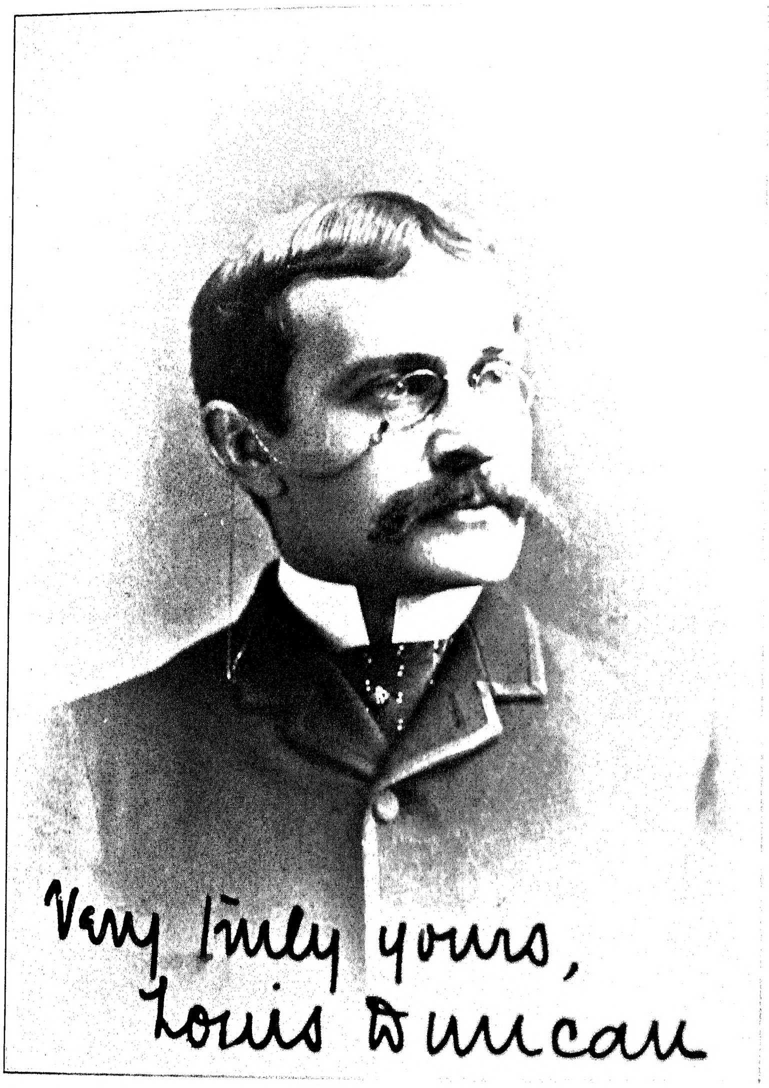
Very truly yours,
Louis Duncan

TRANSACTIONS

OF THE

AMERICAN INSTITUTE

OF

ELECTRICAL ENGINEERS,

(Established 1884. Incorporated 1896.)

VOL. XIII.

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Constitution, Article VII, Sec. 2

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TRANSACTIONS

OF THE

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Vol. XIII. JANUARY TO DECEMBER. 1896.

NEW YORK, January 22, 1896.

The 102d meeting of the INSTITUTE was held this date, at 12 West 31st Street, and was called to order by Vice-President Hamblet, at 8 P. M.

Secretary Pope read the following list of associate members elected and transferred at the meeting of the Council, held in the afternoon.

Name.	Address.	Endorsed by.
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EVANS, EDWARD A.	Acting Chief Engineer, The Quebec, Montmorency and Charlevoin Railway, Quebec, Canada.	Ralph W. Pope. J. Day Flack. Edw. Caldwell.
FRANKLIN, W. S.	Prof. of Physics, Iowa State College, Ames, Iowa.	Ernest Merritt. Edw. L. Nichols. Fred'k Bedell.
GITHENS, WALTER L.	Manager, H. P. Elec. Light and Power Co., 7284 So. Chicago Ave.; residence, 5101 Kimbark Ave., Chicago, Ill.	B. J. Arnold. W. M. Stine. A. S. Hibbard.
IIJIMA ZENTARO,	Assistant, Wagner Elec. Mfg. Co., 2017 Lucas Place, St. Louis, Mo.	A. L. McRae. J. E. Randall. F. G. Schlosser.
LEMON, CHARLES,	Hon. Sec'y for New Zealand for the Institution of Elec. Engineers, Palmerston, North, New Zealand.	Ralph W. Pope. J. Day Flack. Edw. Caldwell.
LLOYD, JOHN E.	Ass't Chief Engineer, Philadelphia Traction Co.; residence, 2008 N. 18th St., Phila., Pa.	F. Uhlenhaut, Jr. Wm. D. Gharky. C. A. Bragg.
MOORE, WM. E.	Electrician and Sup't, The Augusta Railway Co., Augusta, Ga.	A. F. McKissick. A. M. Schoen. J. P. Edwards.

NICHOLS, GEO. P.	Partner, Geo. P. Nichols & Bro., Elec. Engineers and Contractors, 1036 Monadnock Build'g, Chicago, Ill.	B. J. Arnold. Louis Bell. F. B. Badt.
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Approved by Board of Examiners, Nov. 6th, 1895.

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Approved by Board of Examiners, Dec. 5th, 1895.

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THE CHAIRMAN:—Gentlemen of the INSTITUTE, it requires no word of mine to introduce the gentleman who is to read a paper to you this evening. You all know him very well, and have heard him before, and I take great pleasure in having the honor to present to you to-night our Past-President, Mr. Frank J. Sprague.

Mr. Sprague read the following paper:

A paper presented at the 102d Meeting of the American Institute of Electrical Engineers, New York, January 22d, Vice-President Hamblet in in the Chair; and Chicago, January 29th, 1896, Mr. R. H. Pierce in the Chair.

ELECTRIC ELEVATORS, WITH DETAILED DESCRIPTION OF SPECIAL TYPES.

BY FRANK J. SPRAGUE.

There has been so much written on the subject of electric elevators which is pertinent to the subject, that in presenting this paper I shall make free compilation from others, and, supplementing these extracts with some new matter, I shall by lantern views illustrate some details of the more recent machines, their methods of manufacture, and some steps in their development.

The time has passed when any one can doubt that one of the most important applications of the electric transmission of power, and one in the number and variety of its applications already rivaling the electric railway work which has made such marvelous strides in the past eight years, is that of the operation of all classes of hoisting machinery.

Some idea of the extent of the present elevator business may be gathered from the fact that in New York City alone there are not less than 5,000 elevators of various kinds, more, in fact, than there are street cars, and more people are carried vertically than there are horizontally.

Ignoring for the moment the specific methods of application, and discounting the difficulties naturally met in developing machines to do the duty required in modern office work, not alone the technical difficulties, but those commercial ones naturally met when a new company enters the lists with untried machines against the entrenched forces of existing industries, there was still much of encouragement to be derived from a backward glance at the industrial changes wrought in the last few years,

and to all objections raised there came the natural queries: Is the elevator field, great as it already is, limited to the possible application of a water or steam motor? Is there no wider, no more universal application of power for this class of service than has hitherto been presented? Is the hydraulic elevator the one bulwark to stand up against the assaults of the electric giant? Does it present such fixity of design, unity of purpose, refinement of processes, economy of operation and freedom from accidents as to preclude improvement if using some other power?

Let us look at the record in other fields and ask: Why has the trolley system, born only eight years ago, driven the horse from the street? It involves great initial expense, the conversion, distribution and reconversion of energy. It faced all the powers of conservatism, ridicule and fear. It had to combat the allied forces of the Bell telephone interests tested by court action in over twenty States. It had the opposition of the strongest municipal and corporate influences. Every detail of the system had to be created, and yet it stands to-day unrivalled in its industrial progress.

Why is the same trolley system driving the cable to the wall, and why has its adoption marked the abandonment of a plant costing not less than $3,000,000, in the city of Philadelphia? Are there many more examples of the direct applications of force than the cable system, many closer connections between a great engine built for the highest economy and that which it moves?

Why is the steam locomotive giving way to the electric motor on suburban service? Is there any more direct example of the application of steam than is presented by a locomotive, the power of whose cylinders is transmitted directly to the drawbar through the intermediary only of a crank?

Why does the best shop practice dictate the abandonment of the slow speed highly economical Corliss engine and the direct application of power by belts and shafting, and adopt the high speed engine and direct-connected dynamo at a central station, with the conversion, distribution and reconversion of energy by a dozen different motors at a greatly increased initial expense? Is it for any other reason than convenience, reliability and economy of operation?

Why has every overhead crane builder in the United States within the past four years absolutely abandoned wire rope, square shaft and hydraulic transmission for the three-motor transmission

which I advocated only nine years ago? It is because it is simpler, because it will cost less, or because it is more economical, more flexible, and because it answers the purposes better than the other and more direct systems?

Why have the great central stations of the country adopted electricity for the transmission of power to the hundreds of industries within the radius of their supply, and into what form of energy is the power of Niagara being converted?

In short, why is the transmission of power in almost every case where flexibility, convenience, economy, efficiency and reliability are required depending upon the electric method, not only in new work, but oftentimes to the replacing of older plants.

The elevator field, indeed, is a large one, and if the system is electric, then, considered from a commercial standpoint, there appears the following possible classes of work:

1st. High speed passenger service where no hydraulic plant is possible because of limited space.

2d. High speed passenger service in competition with hydraulic plants, the electric plant doing equal duty, costing less, occupying less space than the hydraulic, and costing much less to operate.

3d. Substitution of new high-speed electric service in place of old steam and slow hydraulic services in buildings where the limited space and interference with operations will not permit consideration of a new hydraulic plant.

4th. Passenger elevator service in buildings where the loads are comparatively light.

5th. Passenger service in private houses where safety, simplicity and noiselessness are essential.

6th. Freight and special classes of work.

For convenience we may classify elevator work as "first class, that requiring speeds from 300′ to 600′ a minute," including the first three duties above mentioned, and as "second class," those requiring speeds of from 50′ to 250′ a minute," which include the remainder.

In general, there has been required and developed two kinds of machines to perform these services. The first is the outcome of the increased height of buildings and the demand for high speed and smooth motion, largely regardless of cost of apparatus, space occupied, or cost of operation.

The hydraulic elevator was the result of this demand, and was

the only one that up to a year or so ago was accepted for this service.

It was to meet this demand—by creating an electric elevator which would do the work equally well, if not better, than the hydraulic—that the elevator to be more specifically described, was developed under some unexpected difficulties.

Of course, such a machine must have the speed and capacity of the hydraulic elevator.

It must be absolutely safe.

It should have advantages in the matter of space, and must be more economical to operate.

The second class of elevator work, that which requires lower speeds, is applied to small apartment houses and other buildings where lighter elevator duty is required. This, for a long time, has been fairly supplied by worm gear elevators, and the replacing of the steam engine by an electric motor has enormously broadened the field for this class of machine.

These two machines, however, are not equivalents. They present two distinct kinds of rope movement, two absolutely different methods of control, and two varieties of safeties.

Just here I will briefly outline some of these differences, for they constitute in my mind vital essentials, and are absolutely determinate in their limitations.

The rope movement on the hydraulic is provided by an expanding set of sheaves on which all the ropes are maintained in fixed planes. Four to six ropes can be used on the machine, and six to eight on the car.

The sets of rope can be equalized at the machine, and they have a fixed lead in the hoistway.

The machines can be double and treble decked, and they have absolute limits of mechanical travel.

All of these features are of the greatest importance when dealing with high lifts, large powers and fast travel.

The drum machine, while having a distinct field of its own, and a most useful one, has not a single one of the characteristics mentioned. It cannot well use over two ropes on the drum, and they cannot be equalized at the machine. The lead is a shifting one, and on long lifts this may be as much as from four to five feet.

These particular objections have been met in a type of machine which may be called a cable drum machine, where the drive is by

friction of the rope in the sheave grooves, but in both these machines, the plain drum and the cable drum, there is the very grave objection that there are no fixed limits of mechanical travel which are independent of the armature movement, and on fast speeds particularly, this is absolutely essential.

In the drum machines the driving power is applied through one or more worm gears.

In my own practice on light service, such as house automatic machines, and a low class of freight work, I use a single gear with double ball thrust bearings, and on heavier work, a right and left handed gear generally cut on the Hindley form, to give the fullest amount of gear surface, and with the shafts connected by independent machine-cut spur gearing, which allows the worm gears to be free from each other.

There is another distinction—that of control.

The hydraulic machine is necessarily a gravity machine, using power only in hoisting, its speed on the down side being controlled by the rate of water exit. The machine is, of course, under counter-weighted.

In the drum machine, when there is any attempt at economy, over counter-weighting is generally used, part from the car and part from the back of the drum, the over counter-weighting being approximately equal to the average load.

With these two types of apparatus as precedents, the problem was:

How far can electricity be applied? What are the limitations of control? What the conditions of installation and operation, and to what extent could one type be eliminated?

And the answer is: Both types must be used, but for distinct classes of service.

Taking the drum type and considering electrical control on a machine over-balanced for average service, the load up or down is sometimes with and sometimes against the machine. To control such a machine directly from a supply circuit, (and I cannot seriously consider any other, no matter how ingenious or refined, as meeting general conditions), there is one method only, and that is the use of a rheostat in starting, and the inverse variation of the strength of the shunt field for about a two to one variation in speed. A cumulative series coil is only permissible in starting if variations of speeds are controllable, and in any event these variations are limited. Such a machine is, however, the best for second-class service.

Every one is familiar, of course, with the conditions of ordinary freight work. I might, however, here point out an important branch of this industry, and one which is destined for very wide application, and that is automatic house service, the machine to be controlled without an operator, and so installed as to be as safe as a stairway.

Briefly, such a machine, on my system of working, is equipped with an interlocking switch device on the machine, having a co-ordinating movement with it, and with the controlling circuit in series with a number of door switches automatically opened or closed with the doors. The doors themselves are fitted with mechanical locks, allowing a car to be opened only during a range of movement from 6″ above to 6″ below.

At each floor is a single controlling button. If the machine is at rest, the pressing of a button calls the car, wherever it may be, to the particular floor at which it is wanted, where it automatically stops. When the door is open it cannot be started, and when running, no one else can call it from the floor for which it is destined. The machine also has an additional control in the car, and the safeguards attending its operation are such as to make it safe for servant, nurse, child or invalid.

The development of the multiple screw elevator was undertaken for the express purpose of supplanting in a large way the former standard for high duty office service, and although not by any means an easy problem, either electrically or mechanically, a knowledge of what the hydraulic elevator is, and the variation of the types existing, gave adequate reasons for its attempt.

Let us consider for a moment a hydraulic system, and institute a few comparisons.

It consists primarily of a steam cylinder, or a multiplicity of steam cylinders, working ordinarily under poor conditions of steam economy, that is with a fixed cut-out in the high pressure cylinder of a compound pump, or no cut-off in any cylinder of a simple pump. This element corresponds to the cylinders of a steam engine in the electric system, which use steam expansively with a cut-off varying according to the load, and under pressure conditions which are somewhat better than exist in a pump. It is to duplicate the results of this system of variable cut-offs and steam expansion, that the energies of the various pump builders have been more or less ineffectually bent for a great many years in plain acknowledgement of that defect in their simple and duplex

pumps, the latter of which is common to almost every hydraulic plant of any size in the United States.

It is true that a so-called "high duty" pump with equalizing piston is used on some of the larger elevator plants, but its use has not proven by any means entirely successful, because of the spasmodic nature of the service.

Among the high duty pumps, the flywheel type, such as is used on large water pumping stations has been attempted, but rarely, I may fairly say, with success.

The next element is the water cylinder, which corresponds to the dynamo in the electric system, and on account of the high friction due to the packing, the efficiency of a water cylinder with its valves is not ordinarily equal to that of a dynamo, which with a motor stands to-day the typical example of an efficient energy converter.

The next element is the piping and the tanks, compression or roof, and perhaps an accumulator, into or through which the water is pumped for delivery to the controlling valves of the elevator, and that which corresponds to this in the electric system is its simple wiring, and if a storage battery is used, then this last in conjunction with it.

Any competent engineer knows that, measured by standard practice, a given number of pounds of energy can be delivered to the controlling apparatus of an electric elevator for less pounds of steam, that is, water evaporated, through the medium of no less than fifty combinations of engines and dynamos, than can be delivered to the valves of any hydraulic cylinder through the standard pumps permissible in average elevator service. To be specific, the average water evaporation on a compound duplex pump, which is almost universally used, will in practice, be not less than about 60 to 70 pounds per horse-power of water energy delivered to the controlling valves, whereas the electric combination will easily give the same for less than 40 pounds.

There are exceptional conditions in which a higher economy can be gotten in a hydraulic system, but they are few and are not typical, and under equal conditions the steam consumption in an electric system can be cut in two.

But this is not all. The fact is persistently ignored, although the attempt is made to offset it by recent experiments with a differential piston, that a standard hydraulic elevator uses the same amount of water under the same pressure for every foot of

travel of a car, which volume of water and pressure are determined by the maximum load which has to be carried, although the average load on the ropes, including the excess of car over counterweight, is not over one-third of the maximum. On the other hand, the electric elevator uses, and must use, under normal conditions, current directly proportional to the work, modified in a small degree by starting and slow running.

In short, over and above the friction load of the generating system, the steam consumption in the engines and the generation of electricity in the dynamo, vary with the demand of the elevator machines. It is a system which is of necessity automatic.

On the other hand, the hydraulic system is one of the most flagrant violators of the relation which should exist between demand and supply. It is a system of transmission by water, having at one end a generating plant doing full duty for every foot of travel of its piston, with a variable duty on an elevator car at the other end, and an intermediate straight line water engine with its pipes and tanks taking care of that variable duty, and using the balance of its energy in heating the water which passes through its valves.

Lack of economy, however, is not the only objection to the hydraulic system when looked at from the architect's or builder's standpoint. Until recent developments, these have always been strictly handicapped, not so much perhaps in the matter of cost, but in the internal arrangements of the building as well as in the lay-out of the basement, neither of which could be finally and satisfactorily, if even then, determined, until the particular type of machine had been accepted by the owner, and the contract finally made for it.

Nor has there been either singleness of design or unity of plan of operation. Each maker has had his own form of construction, his special method of control. Every building has brought up a problem more or less new, or at least conditions which had to be seriously considered in determining the elevator service. Horizontal and vertical machines, in basement or shaft; high or low multiplications; long and short, single and jointed cylinders; big and little diameters; large and small sheaves; free and suspended counter-weights; pulling and pushing machines; direct and differential pistons; roof tanks, stand pipes, accumulators and compression tanks; high and low pressures; hand rope, wheel or pilot-valve control; simple or compound pumps—all

have made a nightmare of complications, giving more initial and continuing source of complaint and dispute than all the other engineering problems in a building.

So what more natural than that they should turn to electricity for emancipation? And this tendency is augmented by other reasons.

Leaving out central station supply, always, when properly equipped, to be preferred when the electric service is of a spasmodic or limited character, and considering for the present those large plants which characterize the modern office or hotel building and in a way rival central stations, every engineer knows that the fewer the number of well-proportioned units, the more alike they are, the freer the interchangeability between themselves, and the greater the extent to which any one unit can be utilized, the better the system for power generation and conversion, no matter what its character.

The best modern practice makes a three unit direct-connected engine and dynamo plant the best for lighting a building. There is an empirical relation existing between the number of lights required in a building as ordinarily designed, and the elevator service. When, in addition to the lighting service, such a building adopts electric elevators, it is not now necessary that it shall add an independent generating plant.

All that is required is that the three units should be somewhat increased in size and, perhaps, one of them preferably divided, the mains all taken to a common switchboard with two-way switches, and every engine and dynamo thus made interchangeable on either the lighting or elevator circuits, and at times both, especially if using a slow acting corrective converter, some of each can be run from the same engine and dynamo. So, instead of five or six units, some water and some electric, the entire generating plant is reduced to a single system consisting of three units of one size, or two of that size and two of a half size, which can be run interchangeably, and one of which is almost always in reserve.

Just here it is well to consider the probable application of the storage battery which, if built with plenty of lead, with large surfaces and for heavy momentary discharges rather than for long time steady discharges, will prove a most important adjunct to elevator service, which, like railway work, is spasmodic in character.

A modern office electric elevator on actual average service requires an expenditure of about one kilowatt hour per car mile of travel for every eight or ten feet of platform area. A car will make from $1\frac{1}{2}$ to $2\frac{1}{2}$ miles per hour, so that a battery of six elevators will run from 9 to 15 miles, although very rarely making over 12 miles per hour. With an ordinary car, say from 30′ to 35′ area, this would mean from 3 to $3\frac{1}{2}$ kilowatt hours per car mile of travel, or say 35 to 40 kilowatt hours for a battery of six machines. Without a battery this would require a 120 kilowatt machine as ordinarily rated, worked at an average of 35 to 40 per cent. load. With a properly constructed battery a 60 or even a 50 kilowatt machine will handle the elevators.

Roughly speaking, a storage battery should be able to stand twice the dynamo rate for from three, to seven or eight seconds, and the dynamo rate for one-half a minute. If it has an hour discharge capacity equal to the dynamo capacity in kilowatt hours, it should be perfectly capable to run the Saturday, Sunday and night service required in an elevator plant without losing more than one-half its charge.

So much for the general conclusions on electric elevators, which are necessarily more or less brief.

To meet the hydraulic machine there was designed and developed what is now known as the Sprague-Pratt multiple sheave electric screw elevator, following the general lines of a tension hydraulic machine in the matter of rope movement, limit safeties and method of control.

The net result has been, that this machine now stands the superior to the hydraulic elevator in that it has its speed and capacity with, if anything, greater safety, and certain advantages in its automatics.

On high lifts it occupies less space; it is more flexible in its application, is more economical to operate, and it is more easily cared for.

General Description.—The machine may be described as the combination of two old elements with one new one, with specific safeties and methods of control.

Briefly, it is of the horizontal multiple sheave type, with a traveling crosshead and frictionless nut driven by a screw revolved by a motor directly connected, and governed by a pilot motor and rheostat.

The general construction consists of a heavy main beam, carry-

ing the traveling crosshead and the lower screw bearing, with special castings bolted at each end, one carrying the fixed set of sheaves, and the other the thrust bearing, brake and motor. The regulating apparatus is independent and self-contained, and is placed on the wall. From the car to the system of multiplying sheaves the direct multiplying machine and the horizontal hydraulic elevator are practically the same. The crosshead, however, marks the point of departure in the two types.

In the hydraulic machine, this crosshead is rigidly attached to the end of a rod which terminates in a piston moving in a cylinder having an inside length equal to the lineal movement of the crosshead. This cylinder in the vertical type of hydraulics varies from 30 to 50 feet in length, with from 2 to 12 sheave multiplications, and in the horizontal types the multiplication runs as high as 14, with corresponding diminution of length of cylinder and increase in cross-section. Whatever the gearing, however, the length of cylinder is a function of the car travel. In this electric elevator, the crosshead being moved along a screw, stationary so far as the lineal movement is concerned, there is, with any given number of sheaves--only one variable—the length of screw; and, for all heights above about 100 feet, the electric machine has an advantage in matter of length, which, with increased rises, becomes of great importance.

Looking to the needs of office buildings, there has been adopted two methods of rope multiplication, determined by the height of building, and so selected that the length of machine over all, shall not exceed about 30 feet for rises approaching 300 feet actual car travel. From this the length grades down to about 21 feet, so that all rises between 60 and 300 feet can be taken care of with an extreme variation of nine or ten feet in the length of machine, and there is thus provided limiting dimension data of great convenience and utility.

These systems of multiplication I may term direct and indirect. In the former, the entire multiplication, varying from six to ten, is done at the machine, and the ropes lead from the end sheaves over the shaft sheaves direct to the car. A free counter-weight is used, the ropes being fastened to the car frame. In this method, which is that common to all horizontal and to many vertical hydraulic machines, the hoisting and counter-weight ropes have unequal duty; furthermore, the ropes having the maximum bending, travel on the outboard sheaves at the same speed

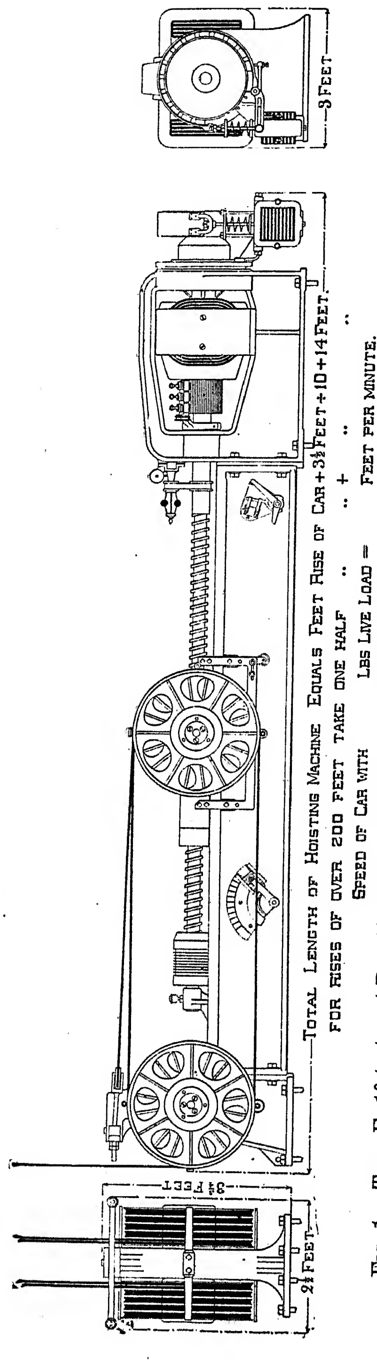


FIG. 1.—Type E, 10 to 1 — 4 R. 10 to 1 = Travel Ratio of Car to Nut = 10 Sheaves. 4 R Signifies Four Hoisting Ropes.

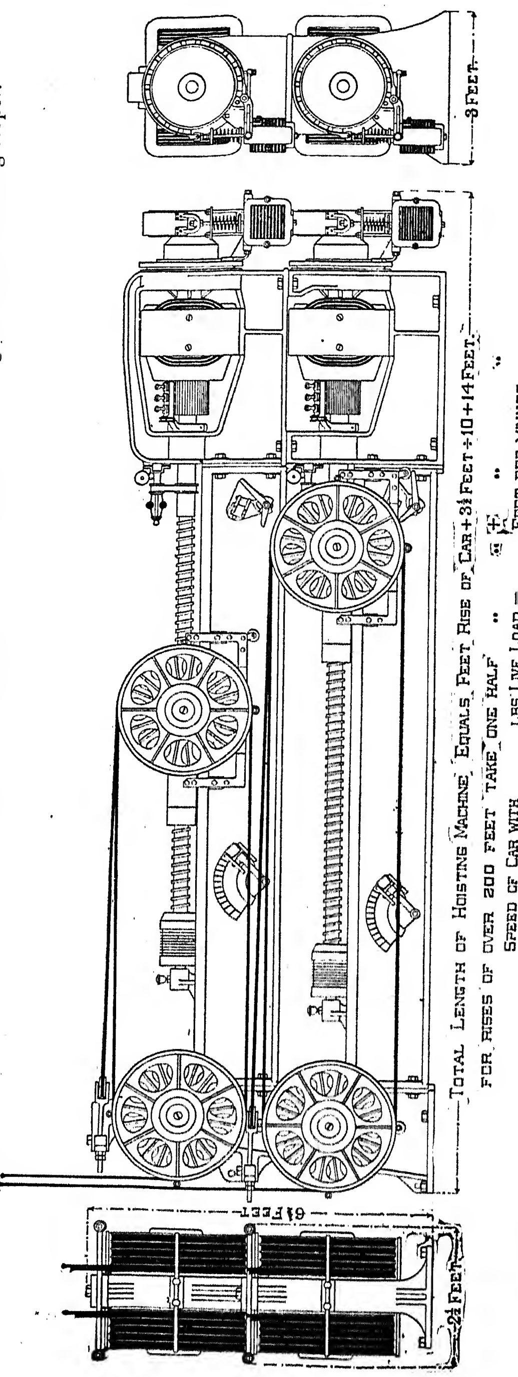


FIG. 2.—Type F, 10 to 1 — 4 R. 10 to 1 = Travel Ratio of car to Nut = 10 Sheaves. 4 R Signifies Four Hoisting Ropes.

as the car. This last is the case also with all vertical hydraulics. In some of the latter, the counter-weight is carried in the cylinder on the piston, or in the strap, or both, its weight being as many times that of a free counter-weight as there are multiplications. Sometimes both methods are used.

Economy of operation and smoothness of movement, however, are antagonistic in their relations to the amount of counter-weight carried. The best method is probably that used when there is a single multiplication in the shaft, giving a two to one counter-weight traveling at half speed, and carried by all the car-hoisting ropes, as is done for short-rise vertical hydraulic elevators.

For long rises I have adopted a combination vertical and horizontal machine rope practice, giving even a more compact machine, a longer life of ropes, and better counter-weight results.

In this indirect system there is a division of multiplication, which, while having the same effect so far as speed of car and length of machine are concerned as a high direct multiplication, has an entirely different result in the wear on the ropes and the amount of counter-weight which can be carried without jumping.

This is accomplished by making one-half the multiplication (6 or 8) on the machine, the ropes, properly proportioned, going thence to the bottom of the counter-weight frame, which has a single multiplying sheave on top. The car ropes go over the shaft sheaves, under the counter-weight multiplier, and back up the hoïstway, where they are anchored, giving a car speed twice that of the counter-weight. The equalizing chains, used to make the pull of the car with any given load constant at all points of the hoistway, are fastened to the bottom of the counter-weight frame and anchored in the hoistway.

The space occupied by this multiplier is only two or three inches more than by ordinary form of counter-weight. A proportionally shorter screw, fewer revolutions, and sheaves of greater diameter, characterize this type of multiplying machine.

This system seems to be the best yet devised for long rises, for not only do all the car ropes do equal duty, both with relation to the hoisting strain and the counter-weight, but the rope wear must be less, because of the division of speed and multiplication, the necessity of changing only one-half of the ropes at a time, and the possibility or reversal of the ropes on the multiplying machine to get a new wear.

Where space is limited, I use a double decked machine, and in

the new Commercial Cable Building, which is to be 21 stories high, the machines will be treble decked, and about $10\frac{1}{2}$ feet in height.

Details.—Turning now to the detail construction and operation of this machine, there are a number of features claiming special attention, each unique in character, and marking a radical departure from all other elevator practice. These are the nut, screw, and thrust bearing, the brake, the motor and the regulator apparatus.

One of the most interesting as well as important features, and, perhaps, the one which has been most frequently attacked, is the nut system. It joins the crosshead of the traveling sheaves by a conical seat. There is no fastening between the nut and the crosshead, the continual weight of the car always keeping them in contact; and the friction at this point, being greater than between the nut and the screw, enables the latter to transmit a straight-line movement to the crosshead when the screw is revolved by the motor, and also to revolve the screw and drive the motor as a dynamo when the mechanical brake releases the screw to allow the car to descend. These are the normal functions of hoisting and lowering. There are several other distinct functions of this nut which will be described in considering the "safeties."

Continuing the line of transmission of power, the only points of contact between the interlocking nut and screw are by a chain of balls which occupy a number of threads, and enter and leave the ends of the nut through tubes which are connected together so as to make a continuous conduit. This is one of the most vital points of the elevator apparatus, and herein lies one of the most potent reasons of its success—the reduction of friction by rolling instead of sliding surfaces on almost all parts under pressure; for not only is the nut so constituted, being in fact a developed spiral thrust-bearing, but the thrust-bearing at the motor end of the screw is taken on balls and the sheaves are carried on ball or roller bearings.

This nut being a vital part, its development has been most thorough, and a peculiar treatment of steel which has been adopted renders its surface so hard that the wear is very small, and it is well within commercial limits.

So free is the machine from static friction that it is possible to start the car with a very slight increase of current over the normal hoisting current, providing time be taken so that the work

done in acceleration is small to the work of lifting, although that is not the usual practice.

The balls have an average crushing strain of 25,000 pounds each, but the working pressure varies from only 50 to 125 pounds per ball.

The nut system is a compound one, for, besides the working-ball nut there is another, called the "safety-nut," to which I will make reference later, keyed to it, and between the two is a powerful spring under compression.

The screw is a forged bar of high carbon steel with a peculiarly shaped thread, the finished screw having a tensile strength of 700,000 pounds. It passes through the clearance hole in the steel trunnion crosshead, which carries the traveling sheaves, then through the nut, and is carried at the outer end by a fixed bearing.

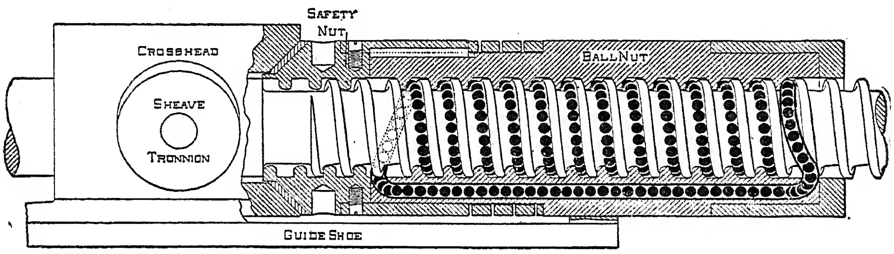


FIG. 3.

This screw is sectioned, being joined to the armature shaft by a cone-seated coupling, secured by a taper gib.

The in-board end of the armature shaft, which is in effect, the extension of the screw, terminates in the thrust-bearing, where the pressure is taken by about 200 steel balls carried in a bronze guide plate and bearing, by specially hardened steel disks. The thrust of the screw being thus taken up on the in-board end, the strain on the screw is invariably between that end and the traveling crosshead—never beyond this; hence, it is always under extension strain—never under compression, and cannot buckle.

The action of the balls on the screw, which is untreated, is peculiar. They form a path for themselves, partly by wear, but principally by rolling compression of the steel, which finally becomes exceedingly hard, such that the edge of any ordinary machine tool would be turned.

The balls themselves wear very evenly. Oblique forms in normal practice cannot exist.

Beyond the thrust-plates is keyed an iron pulley, connected by a flexible coupling with the motor shaft. The function of the brake is that of locking the screw when at rest, it is not a means of varying the speed. In case of accident, it has the additional function of helping to stop the screw. It may be described as a compound electro-mechanical brake. A steel brake band, wood-lined, is anchored at one end, the hoisting-side on the motor-bed frame, and the other end is continually pulled down by a powerful spring under compression. The mechanical movement in opposition is through the medium of a peculiar magnet. It is operated by a dual circuit, one in hoisting, another in lowering. In the event of failure of current for any reason, or too high a speed on the down run, this magnet releases the brake in the latter case by a snap switch, operated by an adjustable Pickering centrifugal governor driven by the main screw—and the brake band promptly grips the brake wheel softly yet powerfully.

Motor.—The motor, which is of the multipolar type, is carried on the same casting which contains the thrust bearing. The field magnets are of steel, and are excited by two circuits; one, known as the shunt circuit, being variable in strength at will, so as to vary the maximum speed of the machine, and the other, a series circuit, which acts to strongly compound the field. This type of elevators is differentiated from all other electrics by the fact that the action is like that of the hydraulic, for it always works against gravity. In hoisting, the motor takes current from the line, but in lowering, its main circuit is cut off from the line, and the motor, rotating in an opposite direction, is driven as a dynamo by the weight of the car. A strong element of safety existed in the fact that the current in the field coils is never reversed, and consequently the machine is never demagnetized. Hence, under certain conditions of the operation of the safeties, it has a power of self-excitation which is of importance.

The armature which turns in this field is of the ironclad type, and not liable to injury of any kind.

It is mounted on a sleeve, is of the 2-path series winding, has a very large commutator, and, of course, multiple carbon brushes. The field coils can be removed without disturbing any other part.

Control.—Considered in its simplest form, and in connection with its action upon the motor and multiplying machine, without reference to the means of communication between the car and the regulator, this last is a very simple device. It is composed of

two parts, each made up of peculiarly shaped iron grids of various sizes arranged in circular form, connected to copper contacts on a slate disk over which passes a heavy carbon brush.

The use of iron castings of a specific composition, possesses great advantage over any form of wire resistance, not alone in the matter of cost. They are flexible, they expand in any direction readily, and, as made, they have a resistance of from forty to fifty times that of copper, or roughly, that of German silver. The grids are interchangeable, and any of them can be readily replaced.

One side of the rheostat is for regulation in hoisting, the other for lowering. Instead of moving this regulator by hand, it is operated by a pilot motor wound with a right and left handed field, one or the other of which only at a time, can be in circuit with the armature. This pilot is connected to the rheostat arm by a single reduction worm gear, and is operated either from the basement or the car, according to the position of the "change over" switch, by an "up" and "down" button with an automatic lever stop which normally has to be held by the operator to prevent the pilot returning to a stop position. The spindle of the rheostat arm operates switches co-ordinating in their movement, in turn controlling the magnetic make-and-break circuit switches, the down brake, and also the automatic stop lever switches which limit the pilot movement.

The use of magnetic switches instead of hand control switches not only removes the arcing from the face of the rheostat, but it gives the benefit of instantaneous cut-offs not possible by any other means of control.

This, on fast machines is of the utmost importance, and the practical application is that if a car is on the "up" motion, and an operator, because of carelessness or because of fright lets go his stop handle, the current is instantly cut off, the regulator following to stop, and the car is arrested in the shortest time practicable.

Assuming that the circuit is made for the up movement, it first meets with a resistance sufficient to about hold the car and lift the brake. This resistance is then gradually cut out, the torsional effort of the armature is increased, giving the car an upward movement with an acceleration depending upon the rate of movement of the rheostat arm, and with a final velocity determined by the point at which the arm is stopped.

If, while hoisting, for any reason the current is cut off, the tor-

sional effort ceases, the brake instantly comes into action and the car comes to rest.

In lowering, the brake is lifted by an independent circuit, but the armature is first short-circuited on itself, and becomes a most powerful dynamic brake. As the resistance in this circuit is increased, the car runs faster. When it approaches the lower limit of movement, an independent retarding circuit is established, and gradually reduces the resistance. This brings the car to the slowest movement.

The pilot movement regulation is, to my mind, an absolute necessity of fast passenger work; and its application, although at first attended with a number of annoying difficulties, is to-day by far the most reliable method of control known.

Safeties.—Of course, the vital question to be considered in any elevator system is that of safety. In that respect, I think, we are amply provided. Safeties may be considered under two heads:

First, those on the car, and second, those on the hoisting machine.

On the car we use a special centrifugal which is attached to the lower section of the car frame.

It consists of two long levers, short-fulcrumed at the sides of the car and operating clamping jaws which run in close proximity to the car rails, but normally out of contact with them. The inner ends of the levers overlap, and in action are pressed apart by a very powerful spring under compression. When out of action, these levers are drawn together, the spring is put under compression, and the system locked by a trigger. Near the trigger is a centrifugal governor, operated by a standing rope, which at a determined speed releases the trigger, frees the levers, and the safeties clamp the rails with a pressure of about 16 tons. This safety can be released from the inside of the car.

In the car, as has already been described, there is an automatic stop contact which operates to bring the regulator to the stop position and the car to rest, in case the conductor removes his hand from the controller because of crowding, accident or carelessness.

On the hoisting machine there are a number of safeties. One which is perfectly apparent is due to the fact that the crosshead is moved by a screw with a heavy armature on the end of it, which is driven through the medium of a nut by a car of limited driving capacity. The screw itself is of forged steel, under ten-

sion and torsion strains, with a safety factor of at least twenty to one.

The hoisting nut, as already described, is hardened by a specific process which makes its wear very limited. In addition to this, there is in the nut system what is called a "safety nut." Normally this is out of contact with the thread of the screw, but it is secured to the hoisting nut, and should any accident happen to the latter, breaking its hold on the screw, this safety nut, the threads of which interlock with the screw's threads to a greater depth than the thread of the hoisting nut, would then take the place of the hoisting nut and securely grip the screw. This would put the elevator out of operation because the friction between the nut and screw would be greater than the friction of the traveling crosshead, and it would act simply as a collar on the screw.

The nut system has in addition another function. Since the hoisting nut is only held from revolving by its friction against the crosshead, when the nut gets to the upper limit of its travel the safety nut meets a solid collar on the screw which stops its travel, causing it and the ball-bearing nut to revolve with the screw, without, however, necessarily stopping the motor, and leaving the traveling sheaves to be stopped simply by the weight of the car.

There is still another function performed by the nut system, that of a slack cable device. If for any reason the car in descending, when of course the nut is driven along by the screw, meets an obstruction, the pressure on the nut being instantly reduced, it recedes slightly, allowing the springs between it and the safety nut to expand, throwing the latter into back contact with the screw threads. The nut system then instantly grips the screw, revolves as a collar, and consequently acts as a check against any marked movement of the crosshead corresponding to a free fall of the car on the ropes.

Assuming, however, the condition of a perfectly free release from all operative safeties, there is a limit to the rate of revolution of the screw, and in any event there is a rubber buffer at its lower end which would cushion its stop so as to prevent any injury.

Besides the lower limit switch, which has already been mentioned, which puts an increasing retarding force on the motor, there is an upper limit switch for cutting off the current; this is

a self-cleaning lock switch, operating in both directions, and moved by a roll on the crosshead. It cuts off the current in hoisting in the upper limit, and allows the brake to come on. On the reverse movement it is automatically closed.

I have already mentioned the governor on the machine, which is called a "monitor centrifugal." This is for operating the brake when running too fast. In hydraulic elevators there is no speed-operated device in case of fast running except the centrifugal on the car, and this is frequently set so much above the normal speed on account of the annoyance of having it operated by a temporary excess, as oftentimes to be useless when actually required. The monitor centrifugal does not throw the machine out of operation, but simply slows it up to any desired speed, and then allows the operator to resume control.

The dynamic action of the machine is made use of in still another way by the introduction of an "automatic choking circuit" and switch operated by the same circuits governing the main brake. It is in constant play and closes the circuit around the armature and its series coils through a rheostat under any of the following circumstances: At each stop from up or down; when running down fast enough to work centrifugal on the machine; on failure of the hoisting current, or on failure of the line current.

So positive is the control over the motor, no matter whether it be operating to hoist the car or retard it in going down, that the brake band can actually be removed and the car still controlled, and even with the brake in normal position the change from one position to another can be made so promptly that it will remain inactive.

Such is the machine which has been developed during the past three years, and whose first application in a large battery in the Postal Telegraph Building seems destined to have the same effect on the elevator industry that the plant at Richmond has had on the railway industry. It is only permitted to me, of course, to make the briefest allusion to this, but as illustrating in some degree the extent of this industry, buildings of from five to twenty-one stories in height are being equipped with batteries of from one to twenty-six machines of various types, and the business of a single company employing some two or three years ago a handful of men, now demands a constantly increasing force already numbering nearly five hundred.

DISCUSSION.

THE CHAIRMAN:—There is no question whatever that the subject which has been before us to-night is one of the most intense interest. Mr. Sprague has shown us what energy can accomplish in a few years, in an entirely new departure in the applications of electricity, and I think many gentlemen present will be glad to learn much more than Mr. Sprague has been able to show us of the subject in the time at his disposal this evening. Are there any questions that gentlemen would like to ask, or is there any discussion?

MR. CHARLES P. STEINMETZ:—I think you will agree with me, that we have listened to a very interesting paper, and especially have seen some very pretty pictures. There is, however, still another side of the question of electric elevators, that of the central station supplying the power. You all know that no satisfactory lighting service can be derived from a railway circuit. Now elevator work is very similar to railway work. It is greatly fluctuating, the elevator taking at irregular intervals very large power and no power, and therefore it is of very great importance to know how much current the elevator motor takes in running, and how much in starting. If the elevator is run from an isolated plant, it will probably be operated from a special generator; but if an attempt is made to operate it from the same generator as the lighting, and the lights flicker every time the elevator starts, in consequence of excessive current taken by the motor, it concerns only the user of the isolated plant.

When operated from a central station, however, the regulation of the system as a whole is of the utmost importance, and, therefore, the first consideration must be, how much power the elevator motor takes in starting and in running.

I am sorry to see that in this otherwise very extensive paper, no data whatever are given of the amount of current consumed by the motor in running or starting with a given load, although these data would obviously be the most interesting part of the paper for an electrical society. In general, I should expect that the gravity elevator would be inferior, in this regard, to an elevator with over-balanced car, since in a gravity elevator the descent is made without power, and consequently during the ascent the motor has to do twice as much work, and thus probably takes twice as much power as an over-balanced car, in which latter case the motor does equal work during ascent and descent. Furthermore, it would be interesting to know whether any attempt has been made to keep the current in starting within reasonable bounds. Obviously the torque required in starting is greater than in running, and an ordinary shunt motor will therefore take a larger current in starting than during operation at speed; which current, if sufficiently large, exerts a vicious influence on the regulation of the voltage in feeders and mains. For

this purpose attempts should be made to design the motor in such a way that in starting it will consume less current than when running at full speed, and still give more than running torque. It would be interesting to know how the elevator motor described in to-night's paper behaves in this regard, and also how elevator motors manufactured by other concerns act therein.

DR. CARY T. HUTCHINSON:—Might I suggest that the remarks of everybody be limited to about three minutes? It is very late.

THE CHAIRMAN:—The Chair is of opinion that the discussion should be limited to the subject presented by the lecturer of this evening.

MR. GEORGE HILL:—I had taken very much the views expressed by Mr. Steinmetz. To a consulting engineer, employed to pass on the question by an architect or an owner as to the relative economy of an electric or a hydraulic machine, this paper, in its title, appeared to be of very great interest. It seems to me, in view of the fact that the electric elevator industry is more than seven years old; that in this town alone there are nearly a thousand electric elevators in operation, the only excuse for describing a special device, which from my point of view is still of questionable utility, would be that it was so economical as to warrant a description of the machine to show why it is economical. Now we have in the paper that has been read before us this evening, not one word as to the facts of the economical operation of the machine.

I have taken observations, a great many of them; I have installed a great many electric elevators and have watched their operation very carefully. I have ridden on the Postal Telegraph machines a great many times. I have observed the generating plant in the basement of the Postal Telegraph Building. I have ridden on a great many high-speed hydraulic passenger elevators, and, although I am a very strong advocate of electric elevators in their place, I affirm that so far as my experience goes, there is absolutely no machine to-day which can compare with the hydraulic high-speed passenger elevator.

MR. SPRAGUE:—The gentleman has just said, and here he rather contradicts an assertion made by another before him, that the electric elevator is not, and perhaps cannot be made, the equivalent of the hydraulic elevator, either in the matter of service or economy. It was naturally impossible in the limits of this paper to go into all the details of the operation of a plant, and I much prefer that they should be brought out in discussion. Elevator service is of a distinct character. It deals with large powers; the requirements are the severest. Human life is the stake, and no manufacturer has the right, whether on the score of economy or otherwise, to ignore that vital fact.

The drum machine, which is the only one which can be over-counterbalanced, and which I briefly described to-night, is one which I very cordially and fully recommend for slow speeds and

limited rises; that is, for a service which can be subordinated to the requirements of the machine and a central station. Its manufacture is not exclusive; it is made by my own company and a dozen or more others. When, however, we come to deal with the elevator question as it has been developed by modern practice, and wish to supplant the hydraulic with an electric elevator, then we must surround that machine with at least all the safeties which exist to-day on the hydraulic plant. It must have absolute limits of travel; it cannot be a drum machine with no fixed limits. It must not be subordinated to conditions which may send it into the overhead sheaves or into the basement. The automatics of drum machines can be so disarranged, and certain electrical conditions may arise which will make that machine on fast service absolutely dangerous. It is true, as Mr. Steinmetz has said, that under certain conditions of over counter-weighting a drum machine can be run with less maximum current demands than a gravity machine. That goes without saying; but, taking my own experience, it is fair to assume that there must be a reason for the existence of the multiple screw machine, since I am building it at the same time that I am building machines of other types.

He says the electric elevator of this type cannot do the duty of the hydraulic elevator. Why? Can it not be run as fast? Will it not lift as large a load? Has it not all the safeties which characterize a hydraulic elevator? It has every required attribute of speed, lifting capacity and safety. In fact, it has more safeties than a hydraulic elevator. As an instance, there is not a hydraulic machine in the world which will permit an operator to leave his regulator with the certainty that that car will be brought to an immediate stop. This single characteristic of this type of electric elevator control would, if capable of being applied to the hydraulic machine, have saved the lives of a great many people in the state of New York in the last year.

He raises the question of economy. I am entirely ready to accept any challenge on that score. It comes down to the number of pounds of coal per car mile of travel, the care and attention of machines, and the depreciation of working parts.

There is not an elevator of any kind in the city of New York that averages over one-third of the maximum load on its ropes, because the public rides to suit itself, and not the economic requirements of a plant. A car weighs from 2,500 to 3,500 pounds, and is under counter-weighted so that the net dead weight, or excess of car over counter-weighted, varies from 800 to 1,500 pounds. The car should be able to carry from 2,500 to 3,000 pounds without being stalled; yet there are very few hydraulic elevators, unless over-pressured, which cannot be stalled by loads which it is possible to put in them. We have never yet stalled a screw electric elevator by any load which it has been required to carry.

The average live load on an elevator is not over 500 or 600 pounds, so that, including the excess of car over counter-weight, the net average load on the ropes is not over one-third of the maximum. Whence goes the energy representing this difference? Heating water in the cylinders. Can it be overcome? The attempt has been made to use a differential piston on one of the largest plants in this city. From an economic standpoint it is an absolute failure, for on the particular plant I have what I believe to be reliable information to the effect that they are using at least four times the coal originally guaranteed.

On another modern plant on which the differential piston is not used, the equivalent work is the pumping of nearly two and a half tons of water a distance about 40 feet greater than the run of the car for every trip, full or empty. Does that represent economy?

On another building in this city, the conditions originally set forth, if followed out by the particular hydraulic system, would require the lifting of a piston and sheaves weighing over 20,000 pounds a distance half the height of the building for every trip of the car, full or empty.

And now about the expenditure of power on electric elevators. On cars of the standard size there will not be used over $3\frac{1}{2}$ kilowatts of electric energy for each car mile in actual average service. The electrical engineers in this room can make their own calculations as to what they can develop that amount of energy for.

The coal burned in the hydraulic systems of this city varies from 48 to 110 pounds per car mile of travel.

The coal burned on the electric system, will vary from 30 to 35 pounds per car mile of travel. Are these statements specific enough?

On a recent test in the Postal Telegraph Building, in which six machines were run absolutely without special boiler preparation, but with all the service of the building excepting the elevator, the house pumping and the pneumatic services eliminated by being operated from the street, 1,163 pounds of coal were burned on 46.3 miles' run, including all three services. With 10 per cent. off for pumping, this was the equivalent of $22\frac{1}{2}$ pounds of coal per car mile of travel. There is not a hydraulic plant of equal capacity in this city which can show such a record.

The amount of coal burned is not the only feature to be considered in a hydraulic plant. It costs money to pack the cylinders of a hydraulic elevator, and on fast speeds, high lifts and high pressures this is a serious item, the cost being all the way from \$15 to \$50 each packing. The good service which the hydraulic elevator has performed during the past years is not a criterion for judging modern high building service, for on such duty cylinders have sometimes been packed as often as every three

weeks for a long period of time, and this can be easily verified by taking the early records in any of the high buildings.

Again, cylinders may pit badly when the water is poor. In the Hotel Netherland, the two main hydraulic cylinders were taken out after a service of practically only eleven months, also an electric drum machine, and the cost of the changes could not have been less than $10,000. It is an exceptional case, and an unfortunate one, but all the same it existed.

The wear on the electric elevator, corresponding to the packing of the cylinder on the hydraulic elevator is on the nut, screw and thrust bearing. Now these screws are not things of very grave cost, but if they wear five or six years, as they will, the annual depreciation, measured in dollars and cents, is not more than the average cost of a single packing of the hydraulic elevator. The operative nuts will wear equally long, perhaps longer. We have not yet worn out one of our standard nuts.

An external central station is not the only source of electricity. Such a system labors under some disadvantages There may be aldermen to look after, street rights to obtain; there is the cost of distribution, meterage and repairs. Every man knows that a central station, so far as the generation of electricity is concerned, is the more economical, much more so than a local or isolated plant can normally be. There happens to be, however, in all large buildings, a number of problems which have to be considered.

Take, for example, a building like the Manhattan Hotel, which I am equipping. They have their lighting and a distributed elevator service with special problems. Sometimes the guests get cold, and the building has to be heated; sometimes hungry, and their food must be cooked; perchance thirsty, and cooling drinks must be supplied, this last is not the only service for the refrigerating machines. A laundry may find a place in a hotel, and bathing is occasionally indulged in. For all these purposes steam must be supplied. Of course, a man can put in his steam ice machines, and he can put in hydraulic elevators with a pair of duplex pumps, and a roof tank or a pair of compression tanks; he can put in his boilers for doing that work, and for supplying the steam otherwise required, and the lighting problem remains. Then he can go to the street for that lighting, but I should be surprised if a competent electrical engineer would hold that this method of dividing supply is as economical as a system of electric generation with interchangeable units, either three of one size, or two of one size and two of a half size.

Suppose one is operating an office building. What do the tenants get? Their light and the elevator service, heat and ventilation. How much light? All they want. That is the great commercial problem here in New York, when dealing with existing conditions,—the difficulty of using a meter in a building where a man expects his light with his rent, and where he goes

out and leaves all of the lights burning, subject to the will of the cleaners.

Hence a man should generate power in his building for all of the service of the building, or take it from the outside for all the service possible. There is a line of demarcation determined by the size of building and the extent of its service. I fail to see how it can help him to get his lights and steam from outside, and pump his water inside. If he stays inside at all, he would better stay inside entirely.

The practical result is, that in the Postal Telegraph Building the number of lights, the car miles of elevator service, the air pumped, the motor converters operated, and the number of hours of service aggregate more for a given amount of coal than like services anywhere in the city of New York.

It is pertinent to ask: Where is this coal saved? In the lights, which are run according to standard practice, in the telegraph service, which is special, or in the elevat r service? Ordinarily, the amount of coal used in a building of this kind for elevator service alone is from 2½ to 3½ tons of coal per day of 12 hours. We can shut off the entire elevator service of the Postal Telegraph Building, and run from the street, and save only an average of a ton of coal in 24 hours, and if a man has to pay a difference of from $6 to $10 a day more for coal for using hydraulic elevators instead of electric, he will find it a continuous interest charge on a rather large investment.

I trust I have answered explicitly some of the questions as to the matter of economy, but if those answers are not definite, I will make them more so.

MR. HILL:—Now let us have the facts. Mr. Sprague says he has lots of them. Let us have those facts added to his paper, and then we will see how they compare with other facts obtained absolutely impartially, and then, if he is right, I will admit it as quickly as anybody.

DR. CHARLES E. EMERY:—Independent entirely of commercial considerations, or the merits and demerits of hydraulic and electric elevators, I will say that personally I am very much obliged to Mr. Sprague for his lecture to-night and for an opportunity to see in detail his admirable apparatus—admirable in design, wonderful in execution. I think that there are many here much of the same mind, and therefore move that a vote of thanks be tendered the lecturer of the evening.

MR. HILL:—I second the motion, Mr. Chairman.

The motion was carried.

MR. H. WARD LEONARD:—As regards the question of power required for starting, and as regards the question of economy in operation,t hose are the two principal points which I personally have been giving attention to, in connection with methods for operating electric elevators. I have not had anything particularly to do with the practical development of ideas of my own which I

have spoken of before on this floor. But they have been nsed by the Otis companies, and plants have been installed which are exactly on the lines of the method of control which I have described to you. This method of control, as you may remember, is one in which—in the case of an isolated plant such as an office building—a generator is in operation for each elevator that is to be run, and the fields of the generator and the fields of the motors are excited from the lighting circuit. I agree with Mr. Sprague in his statement that the power for these electric elevators in large buildings will be obtained from isolated plants, and that there will always be a lighting plant in a building of that nature from which the current can be taken for the excitation of the fields of the motor on my system of control. A plant having three elevators of this nature has been installed in this city which has been recently tested. They are drum machines, and were tested at an operating speed of 450 feet a minute. In starting, the power required is something less than 15 horse-power. As to the water consumption, the Otis company is prepared to guarantee 150 pounds of water per car mile run. The elevator stops by the production of electric energy by the elevator motor acting as a generator, and furnishing its current back to what was formerly the generator, working it as a motor, tending to accelerate the engine. This amount of energy is really quite a large factor, it is no theory. It is an apparent and important factor in the economy of operation. The plant is simplicity itself, as compared with the very ingenious methods and devices which have been shown us to-night, as it has merely a standard shunt-wound generator and a standard shunt-wound motor, and a drum and worm, with an attachment on the elevator drum by which a travelling nut inserts a resistance automatically at the top and bottom of the elevator excursion, to automatically stop the car in its motion. The stop is peculiarly smooth because of the fact that no mechanical brake is used, and the energy, which in the case of the elevator described by Mr. Sprague is used somewhat for braking by using the energy produced by the elevator in its stopping, is in this instance not merely electrical energy produced and then wasted in a rheostat, but a large portion of it is saved as it becomes usefully stored in the energy of the fly-wheel of the engine and becomes available later for operating the elevators. In the case of elevators working on this plan, when operating from a central station, there is between the elevator itself and the source of supply a rotary transformer of energy, which means a mechanism which combines a motor armature and a generator armature. An elevator operating on that plan will entirely eliminate the difficulties mentioned by a former speaker, due to starting elevators upon the Edison three-wire system, which does require a close regulation of pressure on the lamps. There are plants of that nature which have been running two or three years, and there is this to

be said about some ten plants that have been installed on this method of mine, and that is that, so far as I am aware, and so far as I can learn, not one has ever had any portion of its plant replaced on account of breaking, or accident, or wearing out, or from any other cause.

THE CHAIRMAN:—The Chair will announce the discussion closed.

MR. C. O. MAILLOUX:—I would like to hear this discussion prolonged, though not this evening, perhaps. I myself came a long way to hear the discussion, and it is a subject of too much interest to be closed immediately. There may be other statements that other people might want to advance and discuss, and there is a great deal to be said in relation to electric elevators. I might wish to say something myself.

THE CHAIRMAN:—What is the sense of the meeting?

MR. HOLMES:—I move that the discussion be extended half an hour.

MR. HILL:—Before you put that motion, Mr. Chairman, I like to move an amendment to it— that the discussion be deferred to the next meeting.

[The motion as amended was carried.]

THE CHAIRMAN:—Before we adjourn, I have a request from Mr. Steinmetz to read a short communication. I presume he will be very short.

[Mr. Steinmetz read a communication.]

THE CHAIRMAN:—What action will the INSTITUTE take on the communication of Mr Steinmetz?

MR. TOWNSEND WOLCOTT:—Is that in the definite form of a motion?

THE CHAIRMAN:—It is a communication to the INSTITUTE. It is open for action. A motion is in order in regard to the communication.

DR. F. B. CROCKER:—I move that it be referred to the Council, where it should have been sent in the first place.

[The motion was carried.]

Adjourned.

DISCUSSION AT CHICAGO.

THE CHAIRMAN [MR. R. H. PIERCE]:—We are all very much interested in this paper, and we consider ourselves fortunate in having Mr. Arnold, who has considerable knowledge of the subject, to explain it to us. I think that all of us have a great deal of admiration for Mr. Sprague, for we all know the energy, and courage and ability which he has manifested in electric street railway work, and I have no doubt that he is equally efficient in electric elevator work. He was a pioneer in making the electric street railway a success, and he has in the paper brought out a number of good arguments in favor of electric elevators for high

class work to replace hydraulic elevators. Whether or not we agree with Mr. Sprague in all his conclusions, we certainly owe him considerable deference, and ought to respect his judgment, as he has shown himself in the past to be a pretty good authority on similar lines of work. We probably will not agree to many things in this paper. Many statements are made in regard to the inferiority of hydraulic and steam elevators, and some of these we will not assent to. The only thing we have to regret is, that the paper covers so much ground that we are unable to let Mr. Arnold explain it as fully as we would like to have him do, or to go into a discussion such as the paper is worthy of. However, let as many as possible give us their views of this paper to-night, and make what criticism, or ask what questions they can in our limited time.

Mr. Frank B. Rae:—I am not prepared to discuss to any large extent, the paper which has just been presented to us. I think, as our Chairman says, that Mr. Sprague is entitled to a good deal of the credit for the development of the electric elevator. There are a number of things about the Sprague machine which I would like to know more about, before expressing any very decided opinion. There are a number of things connected with this machine, which I am at present inclined to question. The controlling device is one thing which I would like to be more familiar with, and somehow, I cannot reconcile myself to Mr. Sprague's methods in this machine by which he attempts to reach hydraulic practice. I agree with him that it is a good thing to have a mechanical limit to the travel of any machine. It is perhaps necessary to go to to this kind of a machine in order to obtain that limit of mechanical travel.

I would like to have Mr. Arnold tell me the necessity for the large starting currents. Is it altogether necessary that these should be used in the time that acceleration is required to get this machine to speed? There is a reference in Mr. Sprague's paper to kilowatt-hours per car mile of travel of the car. He tries to reduce it to something like the car mile of the street car. I do not see how he arrives at the cost of operation in that way. As I understand it, he makes so many feet, or so many miles of car travel for so much cost of power, without any reference whatever to the load that he carries, or the number of stops that he makes. It seems to me, that the number of stops and starts that are made in the elevator car travel would have a great bearing upon the cost per car mile. It certainly is a fact that you can lift a given load at a given speed with a given expenditure of energy; with a hydraulic machine, or any other, it takes so much energy to lift so much weight. The only thing would be that the less load, the less friction there would be in the lifting device. Every time he starts that car and puts it to speed, he must expend more energy than would be required to run it a very considerable number of feet at full speed. I would like to

understand how he arrives at that particular point of actual expense.

Mr. B. J. Arnold:—In referring to the starting current, there is no doubt that these machines take a high starting current on account of the car not being entirely counter-balanced. We therefore have a very heavy mass which we must accelerate very rapidly. If time was given to accelerate this heavy weight, it could be done gradually with a moderate, current getting it up to speed with a low consumption of energy. It takes a large current, however, to start it quickly, but the objection, such as it is, is largely overcome by the non-consumption of current when the car descends. Many elevators take smaller starting current, but they must use current to descend with, resulting in an equally high, if not higher, average consumption of current per foot-pound of live load carried, than is consumed by the machine under consideration. I have had occasion recently to test three of the leading worm-gear counter-balanced car elevators, and reduced them all to a kilowatt-hour car mile basis.

Prof. W. M. Stine:—Have you investigated the number of stops per mile?

Mr. Arnold:—Yes, I have a record of the stops. The elevator service in leading office buildings is very similar, so that in taking tests of several plants, you get pretty accurate basis of comparison when reduced to a kilowatt-hour car mile basis.

Mr. Rae:—What average of stops would you compute per car mile? Does not every start of the car, does not the number of times you start the car, affect the cost per car mile? What is the average number of stops you are going to allow to the car mile? In considering this car mile cost, the energy expended in starting and accelerating the load, seems to me quite as much a part of the problem as operation at full speed, and as this energy expended varies with the load, I cannot see how it can be determined, except for each individual machine by test, where the cost should be divided at least into two parts, starting and accelerating, and running at full speed.

Mr. Arnold:—In ordinary office buildings, the service is so nearly similar, that the relative tests of different machines give fairly reliable results, and on that basis I have made the tests and compared the different costs, taking into consideration the number of feet each car travels. From these records the energy consumed varied from 4 to 7½ kilowatt-hours per car mile.

Prof. Stine:—To get a correct result, of course you must test at a uniform speed. In taking average service, and that is the only thing you can do, and, at best, your figures under those conditions are not absolutely correct, you cannot get absolutely correct figures on elevator tests. You know they are correct under the conditions in which you test them, but you cannot make a uniform load under which you can make all tests. You do not always get the same basis for comparing the cost per car mile.

Mr. Neiler:—May I ask Mr. Arnold if he has any data concerning the hydraulic service?

Mr. Arnold:—I do not consider that the conditions under which the hydraulic elevator tests were made were very favorable, so it would not be fair for me to state just what the figures were.

Mr. Stevens:—As I understand this paper, one of the main points is the economy of the electric elevator, due to the fact that the motor will adjust itself automatically to the load which the car itself carries, while the comparison is made, that in the hydraulic elevator it is necessary to do the full amount of work whether there be a heavy or light load in the car. Is it not a fact, that the actual work for the varying load of the car is so small a feature in the amount of power required in each case, that it would be rather in favor of the hydraulic elevator.

Mr. Arnold:—No! I think the variable load gives the electric elevators a distinct advantage. In the average hydraulic elevator plant, there is required from 70 to 100 pounds of steam per horse-power hour of work, delivered to the car, while in an electric elevator plant, the same energy is given to the car by the consumption of about 40 to 50 pounds of water, thus effecting a large saving in the fuel required to evaporate the water at first. Again, the electric elevator service has all the advantages that are claimed for an electric street railroad, when being operated on a variable load, viz.:—The absorption of power in direct proportion to the live work done; consequently the variation of the live load enters very largely into the efficiency of the plant.

The point so well brought out by the author of the paper, that as all large buildings now have an electric plant, the electric elevator becomes merely an adjunct to the main plant instead of a distinct plant itself, as in case of the hydraulic, should commend the electric elevator side of this question.

Mr. Stevens:—The hydraulic elevator is usually able to stand side by side with the electric elevator, if the correspondingly advanced ideas of machinery are put in. Very little engineering ability, I think, has been used in putting in pumps for hydraulic elevators. I think a change in the pumps in many of the buildings would increase the efficiency of the plant at least 40 per cent.

Mr. Arnold:—I think we will find a very marked economy in favor of the electric elevator, even when we get there.

Mr. Albert Scheible:—At the Auditorium Annex, in this city, there were some tests made of the hydraulic elevators, count being taken of the number of persons in the car, the number of stops made, and the number of starts, and the steam and water consumption. These tests were made for comparison with some of the Sprague and other papers.

Adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, February 26, 1896.

The 103d meeting of the INSTITUTE was held this date at 12 West 31st street, and was called to order by President Duncan, at 8 P.M.

The President:—The first business before us to-night, after Mr. Pope has made some announcements, is on the motion to incorporate the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

The Secretary read the following list of associate members elected and transferred at the meeting of Council in the afternoon.

ADAMSON, DANIEL,	Manager, John Adamson & Co., Hyde, Cheshire, England.	Chas. F. Sise. Henry Jackson. C. A. Carus-Wilson.
BADEAU, ISAAC F.	Ass't to the Engineer, Met. Telephone and Tel. Co.; residence, 213 W. 121st St., N. Y. City.	J. J. Carty. T. T. P. Luquer. H. Loewenherz.
COLLETT, SAMUEL D.	Engineer Construction Dep't Met. Telephone and Tel. Co., 18 Cortlandt St., N. Y. City; residence, Van Pelt Manor, N. Y.	John W. Howell. J. J. Carty. U. N. Bethell.
DECKER, EDWARD P.	Electrical Engineer, Met. Telephone and Tel. Co., 18 Cortlandt St., N. Y. City; residence, Van Pelt Manor, N. Y.	J. J. Carty. U. N. Bethell. Herbert L. Webb.
FOSTER, SAMUEL L.	Electrical Engineer, Market St. Railway Co., 19 Hobart Bldg; residence, 839 24th St., San Francisco, Cal.	F. E. Smith. J. A. Lighthipe. Louis M. Clement.
GRIFFES, EUGENE E.	Senior Partner, firm of Griffes and Sumner, 307 South Main St., Los Angeles, Cal.	Alfred E. Wiener. Leo Daft. Joseph P. Stone.
MONTAGUE, RALPH L.	Chief of Electrical Department, The Gold Dredging Co., Bannack, Mont.	R. W. Pope. W. D. Weaver. Samuel Insull.
REID, EDWIN S.	Sup't of Construction, Standard Underground Cable Co., 18 Times Building. N. Y. City; residence, 116 W. 11th St.	S. D. Field. M. M. Davis. R. W. Pope.

SHARPE, E. C.	Consulting Electrical Engineer, 20 Potomac Block, Los Angeles, Cal.	Chas. T. Lindner. T. A. W. Shock. W. C. Cheney.
SULLIVAN, EDWARD,	United Electric Light and Power Co., 108 Fulton St, N. Y. City; residence, 337 W. 18th St.	N. Tesla. Wm. Maver, Jr. C. C. Haskins.

Total, 10.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by the Board of Examiners, January 8th, 1896.

SHEPARDSON, GEO. D.	Prof. of Electrical Engineering, University of Minnesota, Minneapolis, Minn.
SINCLAIR, HENRY A.	Electrical Engineer, The Tucker Electric Company, New York City.
NUNN, PAUL N.	Consulting Engineer, San Miguel Cons. Gold Mining Co., Telluride, Colo.
WINSLOW, GEO. H.	Electrical Engineer, 700 Lewis Block, Pittsburg, Pa.
GHARKY, WM. D.	Sup't Underground Cable Const. and Maintenance, Philadelpia Traction Co., Philadelphia, Pa.
FISH, WALTER C.	Manager Lynn Works, General Electric Company, Lynn, Mass.

Total, 6.

THE PRESIDENT:—I would now ask Mr. Vansize to give briefly the reasons why it is desirable to incorporate the INSTITUTE.

Mr. William B. Vansize read the following committee report and explained the points involved.

This Committee was appointed by the Council at its meeting on December 18th, to consider the question of the "Incorporation of the INSTITUTE and the changes that may be necessary and advisable in the Constitution."

The Committee met on January 8th. After some discussion it was decided that it would be advisable to divide the work of the Committee into two parts, and to consider, first, the incorporation of the INSTITUTE; second, the necessary and advisable changes in the Constitution. This was deemed necessary, since the meeting that will be called to vote upon incorporation, must vote unanimously in its favor in order that the action may be valid.

Your Committee believes that it will be greatly to the advantage of the INSTITUTE to be incorporated, and recommends that the necessary steps outlined below be taken to make this change.

A voluntary association, such as the INSTITUTE now is, has no standing before the law and cannot receive bequests or hold property. Every member of such association is individually and jointly liable for all debts incurred by any agent of the association.

A corporation, on the other hand, has a standing before law, it can acquire property by grant, gift, purchase or bequest and hold and dispose of it for the purposes of the corporation. The ultimate liability for debts, for which a judgment against the corporation has been obtained, and remaining unsatisfied, is limited to the Directors, and is not shared by the members of the corporation individually. This responsibility of the Directors is a limited one, and extends only to such debts as are contracted by the Directors during their term of office and which shall fall due or judgment be had within one year from the time at which they are contracted.

It is therefore recommended that the Council take the following steps in order to submit the question of incorporation to the members of the INSTITUTE.

1st. That the Council pass the following resolutions :

"It is voted that the President be directed to call a special meeting of the INSTITUTE, to consider a proposition to incorporate the INSTITUTE under the Membership Corporation Law of the State of New York. (Laws of 1895, Chap. 559, Section 5.)"

2nd. That the Council send out the following notice to all the members of the INSTITUTE :

"Notice is hereby given that at a meeting to be held on the 26th day of February, 1896, at 8 o'clock in the evening, a proposition to incorporate such INSTITUTE or Society in pursuance of Section 5 of the Membership Corporation Law, will be acted upon by the members thereof."

Dated New York, this day of January, 1896.

MAJORITY OF COUNCIL.

This notice must be sent out at least thirty days before the date set for the meeting to vote upon the question, and must be signed by a majority of the Council. If this notice is sent out as soon as possible after the meeting of the Council on January 22nd, the meeting to consider the question cannot be held until the third Wednesday in March, if it is to be a regular meeting of the INSTITUTE.

We recommend, therefore, that the regular meeting of the INSTITUTE, scheduled for Wednesday, February 19th, be postponed one week to Wednesday, February 26th, and that the question be submitted at said meeting.

The laws of the State of New York will make certain changes necessary in our Constitution, should we become an incorporated society. The consideration of these matters can best be postponed until after the question of incorporation is decided.

Your Committee is prepared to do all the necessary work pertaining to the incorporation of the Society, including the legal work.

The Sections of the Laws of New York relating to the matter are added as an Appendix.

An abstract of this report has been submitted to the non-resident members of the Committee, Messrs. Carhart, Hasson and Hibbard, whose names are signed to this report with their authority, thus making the report unanimous.

Very respectfully,

WM. B. VANSIZE, Chairman.
CARY T. HUTCHINSON, Secretary.
T. C. MARTIN.
TOWNSEND WOLCOTT.

By the Secretary. { A. S. HIBBARD.
W. F. C. HASSON.
H. S. CARHART. }

January 21st, 1896.

The Secretary then read the following resolution :

Resolved, That the directors of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS be authorized and directed to incorporate such association, pursuant to Section 5 of the Membership Corporation Law, under Article 1 of such chapter, and to execute and file certificates and take such other and further steps as may be proper and necessary therefor.

THE PRESIDENT:—Gentlemen, the other societies are incorporated, and we are simply getting in line. There is no radical change to be made. Is that motion seconded?

MR. JAMES HAMBLET:—I second the motion.

THE PRESIDENT:—We will take this motion by a rising vote if you please. All the members in favor of the adoption of the resolution will please rise.

[Thirty-nine members rose.]

THE PRESIDENT:—All opposed to the motion will please rise.

[None rose in opposition.]

THE PRESIDENT:—The vote is unanimous. Our next business, gentlemen, is the continuation of the discussion of Mr. Sprague's paper on "Electric Elevators, their Uses and Advantages." According to the notice that was sent out, the discussion will be opened by Dr. Hutchinson, who is now making figures on the board.

DR. CARY T. HUTCHINSON:—Mr. President and Gentlemen, I propose to give to-night a short account of a test that I made on the elevator plant in the Postal Telegraph Building; a test of the energy expended in running the elevators for a certain definite time, under conditions of usual daily operation.

The plant, as has been explained, comprises six Sprague-Pratt screw electric elevators, two for express and four for way service. Current is supplied by a dynamo for this service alone, at a potential of 220 volts.

The test was made by connecting a Thomson recording watt-meter, bought for the occasion and newly calibrated, into the circuit of the six elevators. The readings of this watt-meter give the energy consumed. The travel of the cars was measured by putting a counter on each elevator. The counter was an ordinary bicycle cyclometer, connected so that each revolution of the fast-travelling sheave of the elevator moved the counter one tooth. The ropes lead directly from the fast-travelling sheave to the car; every revolution of the sheave meant a movement of about eight feet of the car; the counters were of course arranged so that they did not register backward; every car, no matter whether it went to the third story or the fourteenth story, registered its travel on the counters.

It was intended originally to have these tests extended over a week, but owing to trouble with the counters, to delay in getting the meter, etc., it was not possible to do this; so the record is for only one day's test, beginning at 10 o'clock in the morning and ending at 6 in the evening. Readings were taken approximately every hour. I believe that there will be no substantial difference between the results for one day and the results which would be obtained in a week's continuous test, at the same time of the year. There should be a difference between the result now, and the result at some different time of the year. The results from hour to hour are nearly the same. I have put a

table on the board, showing the travel of each car in miles, the consumption of energy for each hour, and the result of the whole day's run.

TABLE

SHOWING RESULTS OF THE TEST MADE ON THE ELECTRIC ELEVATOR PLANT OF THE POSTAL TELEGRAPH BUILDING.

Car No.	Car Travel in Miles.						
	9.45 A. M to 11.05 A.M	11.05 A.M. to 12.05 P.M.	12.05 P.M. to 1.00 P. M.	1.00 P. M. to 1.57 P. M.	1.57 P. M. to 2.57 P. M.	2.57 P. M. to 6.00 P. M.	Total for Day.
1....................	1.76	1.14	.90	.97	1.10	3.32	9.19
2....................	1.72	1.10	1.10	1.01	1.10	3.94	9.97
3....................	1.72	1.27	1.10	1.12	1.23	3.85	10.29
4....................	1.78	1.05	—	.22	1.12	3.34	7.51
5	2.33	1.87	1.78	1.84	2.00	5.05	14.87
6....................	2.33	1.87	1.78	1.84	2.00	5.68	15.50
Total..................	11.64	8.30	6.66	7.00	8.55	25.18	67.33
K.W. Hours	49.2	34.3	32.9	32.9	37.	104.1	290.7
K.W. H.P. per Car Mile	4.23	4.14	4.95	4.70	4.33	4.07	4.37

Average speed, 350 feet per minute.
Car floor area, 33 square feet.
Average kilowatt-hours per car mile, 4.33.

There are six cars. Four cars stop at every floor; the others are express, stopping at 11th floor and up to the 14th. Three of the cars usually do not go above the 11th. The others usually go to the 14th. But all these variations are taken account of in the counters, which register the actual car travel, and are not based on any estimate of the average length of the trip. The cars run at an average speed of about 350 feet a minute; the area of the car platform is about 33 square feet. The general character of the installation has been dwelt upon, so it is not necessary to go into particulars.

One further point: Nos. 5 and 6 are the express elevators. In addition to putting counters on the car, I had the conductors register their trips. The counter of car No. 5 gave out after about an hour. In calculating the car travel of car No. 5, I have reduced the mileage of No. 6 in proportion to the number of trips as kept by the car men. The table gives, in the vertical columns, the actual mileage of each of the six cars for each

hour, beginning with car No. 1, 1.76 miles; car No. 2, 1.72 miles, etc., etc. The totals are given in the final column—car No. 1, 9.19; car No. 2, 9.97; car No. 3, 10.29; car No. 4, 7.51; car No. 5, 14 87; car No. 6, 15.50.

The next line gives kilowatts hours consumed, the total being 290.7, given in the column on the right. The total mileage for the six days for the day is 67.33, making the average kilowatt hours per car mile 4.33. The figure for kilowatt hours per car mile varies from hour to hour, on account of the different loads, from 4.07 to 4.95.

The test, I believe is as accurate as a thing of this kind can be made, without extending it over several weeks; it seem to give the results fairly of the actual practice in this particular plant under daily running conditions. I made all the observations myself, took all the readings, and connected all the instruments used; I believe the test is correct in the general result.

MR. J. W. LIEB, JR., (*communicated:*) As this discussion has raised the question of central station service, it may be in point to mention that of 13,395 H. P. on the N. Y. Edison system, approximately 5,000 H. P. are for direct-connected electric passenger and freight elevators, of which there are over 500, probably more than 10% of all elevators in New York. Of these, 360, of about 4,000 H. P., have been installed within two years, of which approximately 200 (2,400 H. P.) were of Otis type; 70 (580 H. P.), A. B See; and 13 (250 H. P. Sprague, the others being of various kinds. In January alone, 50 electric elevators, of over 700 H. P., were connected.

The company has required that the inrush of starting current should not cause a variation in pressure of more than 3 volts above or 3 volts below the 120-volt lighting service, being 6 volts above or 6 volts below the 240 volts between outside wires, on which motors are usually placed. These conditions have, as a rule, been readily complied with by contractors, and the lighting service has compared favorably with that on systems not carrying a motor load. The large generating capacity of stations, and distributing capacity of street network, give central station service in this particular a decided advantage over isolated plants. In the one case, the effect of many motors is balanced and averaged to almost nothing; in the other case the starting of an elevator is shown directly on the lights, which has usually necessitated separate dynamos for elevator service in isolated plants. This seriously increases the investment cost and decreases the operating economy of isolated plants in comparison with central station service. The isolated plant, also, must have capacity to cover the maximum demand of the elevator, except in a building where many elevators are running simultaneously—again a disadvantage compared with the average demand on central station service. Finally, if the elevator service is to continue outside of usual hours, either the isolated plant must be run for excessive

hours at great sacrifice of economy, or a supplemental service must be obtained from central stations, or the elevators must be restricted to the stated hours, to the consequent discomfort and annoyance of tenants.

The price of current for elevator service has been kept at usual power rates—as low, for 1,500 H. P. hours per month, as 5c. per H. P. hour, $6\frac{7}{10}$ per K. W. hour. The intermittent nature of elevator service results in a much lower average return per H. P. from elevator motors than from almost any other class, the figures approximating from $18 to $25 per year per H. P. installed—a cost far below that of a power like steam, which must be kept on tap in the individual building to the extent of the maximum demand, and paid for accordingly. For this price service is available 24 hours for 365 days in the year. These figures and results compare rather well with isolated plants, when all the elements of cost are fairly considered—except in those extraordinary cases, rivaling Keely motor results, where, as in one instance, an engineer reported a cost of less than one cent per K. W. hour for electric current, and another lighted his building for "practically nothing." To enable owners to compare fairly, isolated plant costs with central station service, the Edison Company has recently prepared a blank for the use of owners and electrical engineers. From the data obtained in a recent case, it was found that a proposed building, requiring 5,000 lamps and 2,750,000 lamp hours per annum, would show an operating cost for isolated plant of $18,000 per year, while central station service at present rates would cost about $12,500. It may be added that, central station supply, with the double service usual in large office buildings, from two different mains, and with supply from the several Edison stations, gives a surety of resource not to be expected from an isolated plant, unless under exceptional conditions, and with the large additional investment necessary to give duplicate generating capacity.

MR. GEORGE HILL:—Continuing my remarks at the last meeting, I desire to state that on page 12 of the paper, Mr. Sprague announces: "To meet the hydraulic machine, there "was designed and developed what is now known as the Sprague- "Pratt multiple sheave electric screw elevator. . . . . . "This machine now stands the superior to the hydraulic elevator "in that it has speed and capacity, with, if anything, greater "safety and certain advantages in its automatics.

"On high lifts it occupies less space; it is more flexible in its "application, is more economical to operate and is more easily "cared for."

On page 22 he says that the Postal Telegraph plant is one to which this remark applies.

Mr. Sprague says that at a certain test of this plant 1,186 lbs. of coal were burned, but arbitrarily reduces the amount by 7 per cent., and then says $22\frac{1}{2}$ lbs. per car mile was the duty. You all

know that such a test must have been of relatively short duration; that estimating the condition of the fire at the beginning and the end of the test is very unreliable, and that there is an opportunity for grave errors to creep in; but in addition to this we are not given data as to either engine or generator size, as to time of the day, day of week, number of trips the car made, the number of stops the cars made, whether they were on regular duty or operating by running continuously up and down as rapidly as possible without any stops.

All of this data is necessary to form a correct idea of what can be done, especially when we consider that the amount of current required to start this machine is more than double its running current, and that consequently the power consumption per car mile for the same machine operated under the same conditions will vary almost directly with the number of stops made, increasing with every stop so as to nearly double the current consumption in service conditions over the amount required for exhibition-coal-per-car-mile purposes.

This ends his economy data.

Mr. Sprague gives a few words about tons of water lifted hundreds of feet, $2\frac{1}{2}$ tons lifted 330 ft. per car trip and asks if this is economy. When reduced to H. P., this means 50 H. P. expended during the time occupied by a round trip for one car, regardless of the number of stops or loads.

Comparing this with the figures obtained by Prof. Shepardson at the Minneapolis tests of the screw electric machines, we have car lifting a live load of 2,500 lbs., (small load) 350 feet per minute (low speed), and requiring 52 electric H. P.

In this Shepardson record, no mention is made of the starting current required, so that we have at the beginning an expenditure of energy greater than the figures Mr. Sprague gives us, to which must be added the expenditure of energy required to start the car after every stop.

First: In a first-class elevator plant, there should be:

(*a*) Perfect regularity in car dispatching.

(*b*) No missing of landings.

(*c*) The highest speed the human system can bear.

(*d*) Perfect safety.

(*e*) Reasonable economy, both of capital and for operation as a part of a large plant.

The three first mentioned all have for a common object the delivery of the passenger in the shortest possible time.

The fundamental difficulties with the screw electric machine are:

I. The surge of current through the motor to overcome the inertia of the car is so great as to demand either storage batteries or two separate generating sets. This assertion is based on my experience with the drum electric machines (which require about one-half the current of the screw electric) at the Ameri-

can Book Company and the Jefferson Hotel. Observations of the Postal Telegraph plant, reports of the operation of the screw electric machines at the Hotel Grand (which, by the way, I am told is everlastingly in trouble), Minneapolis and Chicago, and ammeter readings on a dozen drum electric machines here in New York, and observations of the actions of the standard forms of direct connected generating units running underloaded, when suddenly loaded to near their capacity.

If this is not enough proof, visit any of the places where the screw electric machine is in use, and observe the variation in voltage when it starts.

II. The inertia of the car in stopping is constantly varying, since it depends on speed, weight and direction of motion, all of which are variants. The stop is effected by the short-circuiting device and the brake; the short-circuiting device requires time, and the brake having a constant retarding effect, requires a different length of travel to accomplish its purpose each time the conditions vary. As a consequence, either the operator must run with extreme caution, slowing up as he approaches each landing to prevent running past it, or else take the chance of missing the landing and running back to it, which involves a loss of time sufficiently great to permit him to have travelled through another story. This feature is so well known that even laymen mention it to me when I speak of electric elevators. I have observed it in the case of careless operators on every electric elevator on which I have ridden. The defect is inherent in the design of the machine, and increases in seriousness as the square of the speed, and, as a consequence, really high speeds will be found impracticable until a radical change is made.

This feature affects everything except the safety of the machine.

Mr. Sprague says (page 20) that his short-circuiting device can control the elevator, but you know that there must be rotation of the armature and consequently motion of the car for it to be effective.

This cannot be relied on alone, since any break in any circuit of the motor results in throwing it out of operation.

III. Concerning economy; in every first-class hydraulic plant the pumps work at a uniform rate. There are not less than six makers of pumps who will guarantee a duty of 30 lbs. of steam per pump H. P. hour. A plant consists only of pump, two tanks and connecting piping, and the elevator cylinder. The number of foot lbs. of work done by the pump, equals the maximum load multiplied by one half the number of feet travelled, divided by the efficiency, 55 per cent., and is a constant.

The experience of all of the makers tends to the belief that the cars average one-half the number of people lifted that they could lift, and consequently the expenditure of energy in lifting water is about 3.67 times that of the useful work done; but

having once stored up this amount of energy, there is absolutely no limit to the number of stops which can be made. This gives 110.10 lbs. as the steam per useful horse-power, and if the cars all ran fully loaded, this consumption would be halved.

In the screw electric machine, as I have before said, there is demanded either a special generating set or storage battery. For the generating set the engines would run under-loaded, demanding at least 60 lbs. of steam per indicated H. P. for one-half the work to be done, which, divided by the efficiency of the machine 51.3 per cent., gives 117 lbs. as the steam expenditure, plus an additional amount to be allowed for every landing made. This 60 lbs per indicated H. P. hour is based on my observations of about 300 cards taken from seven engines of capacities varying from 25 to 75 kilowatts, showing a steam consumption by computation from the cards, according to the Thompson method, of

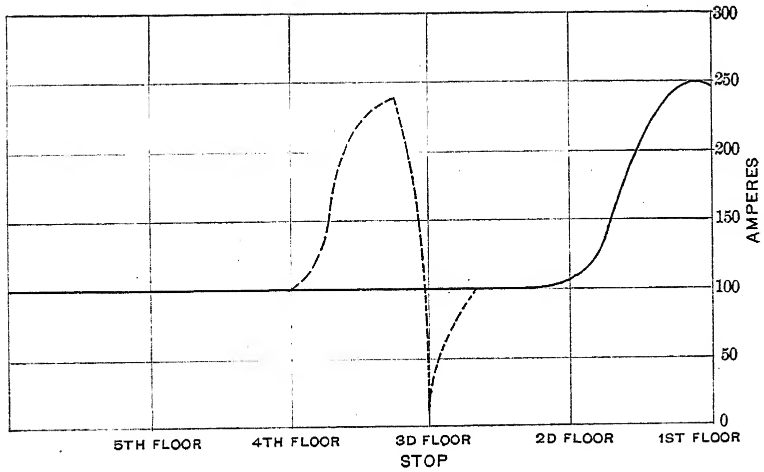


FIG. 1.

from 67 lbs. at 25 per cent. of rated capacity, down to 30 lbs. at rated capacity.

The influence of the number of landings made, and the care with which each stop is effected, has an interesting effect on the output, thus the current for one machine making a run of the entire travel with but one start would be as shown in Fig. 1. The hump must be repeated every time the car is started, and every time a landing is missed. Imagine the consequence on the area of the curve where a car is compelled, as in many of our buildings it is, to stop on an average of every other landing, both up and down. Our economy, as above stated, was for a single start. For the usual conditions the steam consumption must be very much increased.

If a storage battery is taken as the other alternative, an entirely new set of conditions arise.

In the first place we have the interest and maintenance of the battery to be charged against a saving of coal, and in the next place we must consider the efficiency losses of the under-loaded generator, charging the batteries through a booster, 3 or 4 automatic switches, etc.

In the case of the Postal Telegraph plant, to install a battery, allowing 15 per cent. for maintenance cost, depreciation and interest on investment, there would be a minimum charge of $3,000 per annum, or enough to purchase more than three tons of coal per day—rather a serious item.

Assuming that the storage battery is used, and admitting that by its use the coal consumption could be halved in so far as the storage of energy is concerned, we have to consider the watt efficiency of the battery, a matter of about 85 per cent., and, next, the efficiency of conversion of the booster, say 90 per cent., and a steam consumption of 35 lbs., or a resultant consumption of 90 lbs., or 20 lbs. gain, considering simply efficiencies. Of economy we can then say that for a special generating set, the steam consumption will be about the same (110 lbs. hydraulic—117 electric) when the conditions are best for the screw electric, and worst for the hydraulic, as I have before demonstrated.

For storage battery use, we have seen that the cost of battery would about pay for the coal which Mr. Sprague says is required to run an hydraulic plant. (Discussion page 26). If we add to this expense the cost of a break-down connection, the cost is nearly double again.

Second. A few words concerning some of Mr. Sprague's other points:

On page 5, Mr. Sprague gives the field of the electric elevator. His first sub-division brings up a condition that has never occurred in my practice, and, in my belief, could not occur except in connection with other conditions, which would dictate the demolition of the building entirely.

His second case I dispute.

His third case I cannot imagine as occurring, especially as I know of several cases where a change from a slow hydraulic to a high speed hydraulic was effected without interference with the service.

His fourth, fifth and sixth cases are perfectly met with the drum machine.

His statement on pages 9 and 10 is in my estimation unfair, since, as I have above stated, in a reasonably well designed plant, properly operated, there is a demand for a constant output of energy on the part of the pumps, and it is simply a question for the owner to determine between capital account and coal bills.

The statement on page 9 that fifty combinations of engines and dynamos demand less water evaporated than one hydraulic

pump is a fair sample of the statements made. Taking his own statement as to the steam consumption of a generating unit as given in the same paragraph, as less than 40 lbs., say 30 per H. P., and taking the efficiency of the combination as 95 per cent., we would have for fifty transformations 390 lbs. of water to compare with the 60 or 70 lbs. stated by him in this paragraph as the pump water consumption.

On page 10 he says the electric elevator used current directly proportioned to the work, modified in small degree by starting and slow running. I think, on the contrary, it would be more nearly the truth to say modified in an enormous degree by starting, slow running and number of landings.

The difficulties mentioned in the last two paragraphs of page 10 are, as far as my experience goes, fanciful, due not to difficulties inherent to the hydraulic elevator but simply to the disinclination on the part of most architects to be bound by limitations of any kind whatever.

Concerning the packing of hydraulic cylinders and the cost of maintenance of them, Mr. Sprague mentions isolated cases where trouble has occurred due to stupidity on the part of the operators, or carelessness or worse on the part of a shop foreman. A most cursory investigation of the hydraulic elevators will convince the fair-minded that the difficulties are exaggerated.

Mr. Sprague mentions the need of heating. I cannot speak for every consulting engineer, but I know that in my own practice one of the first points which I consider is the heat unit transmission of a building for the year, and I usually figure it out very carefully from the best plans available, using the German method and apportioning the losses per square foot of wall, window, roof and cellar surface according to the actual conditions.

In the American Book Company plant, we find it practicable to heat all of our part of the building under all conditions, and all of the three upper stories occupied by the University under all but the most severe conditions with less than three pounds of back pressure.

At the Odd Fellows Temple in Philadelphia, we heat under all but the most severe conditions, without one pound of back pressure, and without the use of live steam.

In both of these large buildings, there are two high speed hydraulic elevators operating constantly, during seventeen hours a day.

In a great many other plants that I know of, the daily coal consumption increases in the fall and winter as a regular thing, not because of the increase of lighting requirements, since my observations show it to vary with the outside average temperature rather than with the condition of the sky or ampere output, and I have the records of three large buildings to refer to.

Third. Let us refer to the Postal Telegraph plant:

Mr. Sprague, in his very interesting remarks, states that "in "the Postal Telegraph Building they are running more lights, "more car miles of elevator service, pumping more air, running "more motor converters, running more continuous hours of ser- "vice in a year than any building in the city of New York, "and running it on less coal than any building in the city of "New York for the same service"

This is a general challenge to produce any mechanical plant of nearly equal size operating as economically.

There is also the inference that this economy of operation is due to the use of the Sprague-Pratt screw electric elevator. I shall show that this is a very inefficient plant.

Recently it has been burning on an average of $7\frac{3}{4}$ tons per day of broken coal at $4.50 per ton. It has one generator running for electric elevator operation alone, in which the amperes vary from zero to 650. Volts vary from 190 to 245. The elevator speeds are about 275 feet per minute.

My authority for coal consumption cannot be divulged, but I pledge my word that you would unquestionably accept it if I should state it.

PHILADELPHIA RESULTS

Location.	Elevator.	Rise.	Trips.	Total Travel, Feet.	Car Miles.	Coal. Total Tons.	Coal. Per Car Mile, lbs.	Other Work.	Service.	Test Made.	Remarks.
Drexel Bldg.	1	60	603	72,360				One boiler running elevator pumps only.	Regular daily.	Every day conditions.	22,000 people carried as daily average.
	2	60	535	64,200							
	3	120	300	72,000							Net capacity of one car, 1,500 lbs.
	4	120	453	108,720							
	5	120	418	100,320							
	6	120	365	87,600	95.8	2.11	44.2				
305-307 Walnut St.	1	66	—	—		15	tons	House			Capacity, 2,000 lbs.
	2	58	—	—		per	month	pump.			" 1,500 "
Trust Co. of N. America	1	77	250	38,500					Regular		Coal estimated by engineer, average.
	2	58	250	29,000	12.8	0.51	79.8	Supplied from one boiler.	Daily.		
Land, Title & Trust.	1	78	300	47,800					Daily service	Every day conditions.	
	2	78	300	47,800							
	3	95	150	28,500	23.2	0.47	41.0				

All are a good type of hydraulic elevator with the ordinary pumps, some compound and some simple, with no attempt made to secure high duty. A simple change in style of pump would reduce the coal per car mile to below 20 lbs.

RELATIVE FUEL COSTS.

FEBRUARY COAL BILLS.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
										Hours Service.						Hourly Fuel Cost				
Building.	Length.	Breadth.	Height.	Court Area Sq. Ft., Est.	Net Area.	Cube	Tons Coal Daily.	Price.	Coal Cost per Day.	Act Full	Act Partial.	Equi. Full.	Est. Full	Special Allowance, per cent.	Cube Cost.	Per 24 Hours.	Column 14 Basis.	Column 15 Basis.	No. of Elevator.	Exposure.
Postal Telegraph Building ...	150	75	215	1,200	10,000	2.15	7.75	4.50	34.84	12	12	4	16	+ 10	16.20	0.67	1.01	1.12	6	S.
Home Life Insurance	125	55	240	1,3[illegible]	5,5[illegible]	1.3[illegible]	3.18	2.75	8.75	11	6	2	13	—	6.6[illegible]	0.27	0.51	0.51	3	N.
" " "	—	—	—	—	—	—	4.75	2.75	13.05	11	6	2	13	— 15	9.88	0.41	0.76	0.64	—	—
Manhattan Life....	115	[illegible]	250	1,000	6,400	1.[illegible]	6.50	2.75	17.[illegible]	11	13	4	15	— 20	10.79	0.45	0.72	0.57	5	N.
Mail and Express	Special.		140	780	7,[illegible]	1.1[illegible]	4.00	2.75	11.00	12	—	—	12	— 25	10.00	0.42	0.83	0.62	4	N.
Morris Building	98	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible].43	2.75	[illegible]	10	[illegible]	1.5	11.5	+ 25	3.79	0.15	0.33	0.39	3	N.
Am. Bank Co.	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	3.5[illegible]	2.75	[illegible]	12	[illegible]	—	12	— 15	3.60	0.15	0.30	0.25	7	N.
Odd Fellows Temple ...	17[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	2.1[illegible]	[illegible].22	2.75	[illegible]	[illegible]			18	— 20	4.0[illegible]	0.17	0.23	0.18	2	N.

Cube stated in millions of feet. Cost in dollars per million cubic feet. Column 19 gives fairest comparison.

The Postal Telegraph Building is protected by the Hom Building on the north. Its exposure is principally t south, affording an advantage of 20 per cent. in heat req The elevator service is not 24 hours a day for the entire but that of a regular office building, with a few of the ele a portion of the lights, etc., run at night for the telegra vice. The building is very well lit naturally.

This coal consumption is for a very recent period. Sprague's figures, given after the conclusion of my rema an average from June '94 to December '95 of 6.2 tons, a practical substantiation of the figures above given, beca gives us the average for two summers and one winter, ing from the time when the building began to be occupied records of plants show about the same proportionate in between the daily average during the period for which I made the comparison.

The Home Life Building is exposed on all sides, is ve and has a tower, which I have not counted in making th It has an expensive and extensive indirect heating syst the lower floors, three hydraulic elevators, is occupied as fice building during twelve hours of the day, with a supple service for five hours longer. I give two rates of coal con tion, the lower rate being for five degrees higher a temperature than was experienced during the time Postal Telegraph records under an exceptionally compet gineer, and the other taken at the same time that the Pos servations were made, and the work of a less compete gineer. My belief is that more than half of the differ due to the difference in the men.

The Mail and Express Building is exposed, as you kn all sides. In addition to heating the building, ventilati basement with a large electric fan, ventilating the engine with a large steam fan, operating four hydraulic vertical der elevators, two hydraulic horizontal cylinder plate hoi entire lighting service of the building and the operation large Hoe presses required for the issue of the paper, co able live steam is used in the stereotyping room for the of a small engine running eight hours a day, and for the of a building adjoining. The plant unquestionably co materially improved; the daily coal consumption, ho shows remarkably well.

The Morris Building is strictly an office building, runn night for the convenience of tenants, as well as by day, ha hydraulic elevators and an independent electric lighting coal consumption given is annual, averaged for a day.

The American Book Company building is ten storie cellar and sub-cellar, nine of the stories being occupied American Book Company factory, in which there are over 1 of motors, three large drum electric elevators, two hydrauli

tors of 450 per minute speed, one steam and hydraulic sidewalk elevator, a wasteful indirect heating plant for the roof, requiring live steam in severe weather in considerable quantities, a number of ventilating fans, etc. The hydraulic elevators are used for the service of the three upper floors, and are run constantly. Inasmuch as the shipping departments are on the third and fourth floors, and the manufacturing departments on the top floors, the electric elevators are in constant use all of the time. Two points are particularly noticeable. One, very moderate coal consumption, dropping to less than two tons of coal per day in the summer, and the other, the fact that the wattmeter records taken continuously day after day, one on the manufacturing circuit, which includes all fans, etc., and the other on the total power, which includes the first circuit and the electric elevators, are practically only 2.5 K. W. apart, with an occasional peak lasting but a second when two or more elevators happen to start together.

The Odd Fellows Temple is a large building, one-half occupied as offices, and the other half for Odd Fellows' meeting rooms, entertainments, etc. One of the courts only goes through five stories; the building is heated entirely by indirect radiation, a 120-inch Sturtevant fan being used occasionally during the daytime, and always during the evening in the summer. The building has two hydraulic elevators, besides its isolated plant, and runs continuously for eighteen hours a day. In the top floor there is a restaurant, which, the janitor claims, demands the expenditure of one ton a day alone for live steam for cooking, etc. The coal consumption stated is for the same period as the Postal, and is about the average in addition for the three winter months.

The Manhattan Life has 5 hydraulic elevators; one running twenty-four hours a day, has the Weather Bureau, which demands light and heat during twenty-four hours a day, and a seven-story tower, neither of which are figured into the cube, nor allowance made therefor. The regular office service is for eleven hours a day, and there is a supplemental service of thirteen hours. Coal consumption stated for the same period as the Postal. Elevator speeds are very much higher, and my belief is that the elevator car miles exceed those of the Postal by probably 30 per cent. Lighting requirements are undoubtedly greater, and the heating requirements certainly so, as the building is entirely exposed on all sides and is very lofty.

I have endeavored to make the table giving these results as complete as possible.

In the column headed "Special" I have made allowances in accordance with my own judgment, so as to reduce all to a uniform basis of hours of service. The two other columns of cost are figured from a perfectly evident basis.

Taking any one you please, the marked inefficiency of the Postal plant is evident.

In conclusion, I am fully alive to the shortcomings of the hydraulic elevator, have been fighting for years to obtain more efficient pumps in my plants, and to get a closer relation between the energy expended and the load lifted; but I do not see what road is open to secure the enormous advantage inherent in a hydraulic cylinder due to the fact that the acceleration of the car on starting does not derange the source of supply while in every form of electric elevator, this must occur. It can be handled in the drum machine, but not in the screw machine within any reasonable limits, and the final point, that the stop in the hydraulic machine is always positive, while in the electric mamachine it rarely is.

MR. JOHN D. IHLDER:—I should like to call your attention to the difference between the screw machine and the ordinary drum machine. Mr. Sprague makes the claim that the screw machine is the only one that is fit for high speed service, and he bases his claim upon its special construction. Now, why an electric machine which has a rotary motion should be built on the same lines as a hydraulic machine which is reciprocating in its motion and has a short cylinder, and therefore must have multiplying sheaves, seems very strange. The screw machine of Mr. Sprague has, through these multiplication sheaves, an enormous pressure on the screw, which it seems he has overcome in a more or less satisfactory way, by making special tools, using special material, and large ball bearings, all of which are not necessary in the ordinary drum machine. The ordinary drum machine has the large advantage, again, of overbalancing. The screw machine cannot be overbalanced. In this way the drum machine reduces its duty down to less than one-half that of the screw machine, consequently the screw machine requires a very large plant for operating it, or has to use very heavy currents when it draws its power from the street. The difference between a balanced car and an overbalanced car is not very familiar to most of the members here. To illustrate this difference, take, for instance, a car of a duty of 2,000 pounds, with 400 feet speed. A drum machine can overbalance this load, if it is desired, to one-half, so that the duty of the machine is reduced down to 1,000 pounds. But the screw machine of Mr. Sprague's cannot only not overbalance the load, but, in order to start and accelerate the empty car on the down trip, is obliged to leave the car at the lowest figure 500 pounds, probably 1,000 pounds heavier, so that the duty of the screw machine is 2,500, or 3,000 pounds, instead of 1,000 pounds, for the drum machine. This is evidently a very great disadvantage against the screw. The only points that Mr. Sprague brings out to advance his claim that the screw machine is the only one that is fit for high speed are that it has a limited motion, and that the lead of the ropes is always in the same line; it does not shift as it does on the drum. Now, the shifting of the ropes on the drum is practically immaterial. It does not make the slight-

est difference whether the ropes vary, in their lead in 150 feet, something like two feet, or, when it comes to a 300 feet building, whether they vary something like four feet. The limited motion which is claimed as a parallel to the hydraulic machine is incorrect. However, there is practically no difference whether you consider a drum machine or Mr. Sprague's screw machine. Mr. Sprague tells us himself that it is impossible to suddenly arrest the motion of the screw. If you attempted to do it, something would break. There is so much power contained in the motor and the momentum of the car pulling ahead, that it would be impossible to suddenly arrest the motion. Consequently he must cut off the current and gradually produce friction, and in that way bring the car to rest. In its general features, the drum machine has the same arrest of motion. Most of the designs which are at present in use have a thread cut on the main shaft of the drum, with a traveling nut. It sets a train of gears in motion and cuts the current off, the same as the nut does in Mr. Sprague's machine, and puts a brake or some other arresting device on. So that the difference is practically only one in design; in theory it is exactly the same. Where the great difference exists that makes one machine fit for high speed service and the other not, I fail to see.

MR. H. WARD LEONARD:—I occupy rather a peculiar position. I do not think that the hydraulic machine has the superiority over the electric machine which has been claimed for it by its advocates. Neither do I think that the type of electric elevator which Mr. Sprague has described to us has the adwantages which he has claimed for it. But I do believe that the electric elevator in its best form has advantages over both the types that have been discussed. The hydraulic elevator has many features of disadvantage, which I think Mr. Sprague has put very forcibly. The Sprague electric elevator has a great many disadvantages which I think the hydraulic people have set forth very clearly. The desirable features for an electric elevator are that it shall be cheap as regards first installation; that it shall be cheap as regards operation; that it shall be possible to operate it at quite high speed under satisfactory control; that it can be stopped and started smoothly; that it can make a perfect landing; that it shall be quiet in operation; that it shall be simple in its essential parts; that it shall be clean, which is one of the points that is quite a factor in the office buildings of to-day, and in which electric elevators have a decided advantage over hydraulic elevators; that it shall restore useful energy in stopping; that it shall require but a small amount of power in starting—that is to say, a power which is proportional to the actual work without the necessity of the power which is absorbed and wasted in rheostats or any other starting devices of similar character;—that it should not affect electric lights which are connected to the same source; that it should require but a slight at-

tention to keep it in perfect order; that its cost, due to depreciation, should be small, and that the cost of power, in case power be purchased for operating it, should be small.

I believe that elevators running on my system of operating elevators have these features to a more marked degree than either the Sprague elevator or the hydraulic.

A statement of that kind carries but little weight, and I have not authoritative figures to substantiate it. But in case any person has a desire to investigate whether such a statement has any foundation in fact, I can refer them to the last installation, which is in Fahey Building, in Maiden Lane, where three elevators of the drum type are installed, and where we have no difficulty in operating to about the same height as the Postal Telegraph elevators. They have been in operation about three months. They start with a much smaller amount of power than any elevator that starts by rheostatic control or any other means. They do restore energy in stopping. They are perfectly noiseless in operation. The motion is perfectly smooth in starting and stopping. They make a perfect landing. The machinery is perfectly clean, and the speed is higher than the speed quoted for the Postal Telegraph elevator.

Mr. Franklin S. Holmes:—I have watched the Sprague elevator for some two or three years with much interest. I feel that any person who has the grit to build a machine on new lines and perfect it as that machine has been perfected is entitled to the thanks of the mechanical community.

But there seem to be two objections to the operation of a motor on these lines, and, indeed, to the electric elevator as it is now operated. The first is that we are taking a motor at its weak point. We take a motor standing still, and we ask it to lift a load at a speed equal to 400 feet per minute. Now, we know well enough that the current taken by a motor under these conditions is twice as great as the current required to operate it when in motion. Any machine that requires two or three times as much current to start it as to run it, simply because, or largely because, of a weakness of the machine itself, is fundamentally wrong. There are people who are now trying to make a combination between the hydraulic and the electric elevator thereby avoiding this weakness of the motor. In these days when pure breed is valued so highly, possibly such a mongrel as that is looked upon a little askance. But I believe that along these lines we shall, with direct current—I am not speaking now of alternating current apparatus—get the best results in elevator service. The objection which is peculiar to the Sprague elevator, it seems to me, is that it does not allow over-counterweighting. Mr. Sprague divided elevator work into two parts, namely, medium service and high duty service, and he claimed that the only elevator which can operate successfully on high

duty service was the screw elevator. I do not think that point was established. I think that the drum can be made serviceable for high duty service. The Sprague elevator takes about 200 amperes of current; I believe, to start, and a third of that, about 70 amperes of current, to run. The Otis elevator takes about 80 amperes to start and about 25 to run. Now, I have been on the platform of an elevator, where the armature was turning constantly, which took to lift three men, at the rate of 150 feet a minute, 30 amperes to start and 20 amperes to run, at 110 volts, which means 5 horse-power to start and 3 horse-power to run. This was under conditions when the current taken to come down was the same as that taken to go up. In other words, the thing was perfectly balanced. But that is the advantage you get when you have an armature that is turning constantly in one direction, and is one thing we are looking for. Such a machine will be commerical before a great while.

Dr. Hutchinson:—Mr. Leonard says he has not figures to prove some statements about his elevators. If Mr. Leonard will give me permission to quote some figures that I have knowledge of, I will do so.

Mr. Leonard:—I do not know what the gentleman refers to. But any figures he has he is at liberty to state.

Dr Hutchinson:—I refer to a test of an elevator running in the Otis factory, when you were present.

Mr. Leonard—Do you refer to the apparatus you designed s

Dr. Hutchinson:—No, I refer to the apparatus that the Oti? company designed and use in their factory.

Mr. Leonard:—I was never present at any test where you were present except of an apparatus you designed.

Dr. Hutchinson:—I beg your pardon; we ran a test in their factory on their motors.

Mr. Ihlder:—I think we made a few tests. Possibly I have the records in my test books, and they are at your service.

Dr. Hutchinson:—I do not want to give the figures without Mr. Leonard's permission.

Mr. Leonard:—I am perfectly willing that they should be given, with the explanation that they are not of the kind of plant that I mentioned when I spoke.

Mr. Ihlder:—The figures taken there were from a dynamo driven by a motor, and then the dynamo drove the elevator motor; the plant which Mr. Leonard refers to has a dynamo driven by a steam engine. It leaves out the motor.

Dr. Hutchinson:—Precisely; the steam engine takes the place of the first motor.

Mr. Ihlder:—The steam engine takes the place of the motor. Those tests were made some time ago by students of the Stevens Institute, who will publish them in the near future.

Dr. Hutchinson:—I am simply referring to tests that you were present at.

Mr. Ihlder:—If you have any, they are entirely at your service.

Dr. Hutchinson:—It is merely a question of the current required to run the intermediate dynamo and motor constantly, whether the elevator is moving or not. This work was done under your direction, and I shall not quote it except by your permission.

Mr. Leonard:—With the explanation I have made, you can give any figures you want to.

Dr. Hutchinson:—The readings I am about to give were made on a slow speed drum freight elevator in the Otis factory. The car was driven by a motor of about 15 horse-power, to which current was supplied by a dynamo, which in turn was combined with and driven by another motor, supplied with current from the building circuit. The strength of the field of the dynamo, and consequently its voltage, was varied by rheostat, controlled by the operator on the car. The first motor and dynamo ran continuously during the hours of service of the elevator. In stopping, the elevator motor drives the dynamo as a motor, which in turn causes the first motor to act as a dynamo, thus returning current to the line

The readings were as follows:—

Current going up	20 amperes.
Current coming down	40 amperes.
Starting current	80 amperes.
Current when car was stationary	16 amperes.
Current returned to line in stopping, depending upon the suddenness of the stop, from 20 to 60 amperes.	

The average of the up and down current was 30 amperes. Of this, 16 amperes is required for the dynamotor. That is to say, over 50 per cent. of average power expended on the car is used in the controlling device. The actual running time of such a car is about one-fifth of its nominal operating time. With an elevator service of ten hours, 160 ampere hours would then be required for the dynamotor, and 28 ampere hours for moving the car. That is to say, of the total energy expended, only 15 per cent. goes to useful work, the balance being wasted in the dynamotor.

When a steam engine is substituted for the first motor, the case is worse, since a motor is a much more proficient piece of machinery on light loads than an engine.

Mr. Ihlder:—I believe it is hardly fair to Mr. Leonard that this plant, which was run experimentally, should be quoted as giving exact figures.

It was a motor generator combination running 3,000 revolutions, which was put together by me out of machines which we happened to have in stock, in order to demonstrate that a speed of 3,000 revolutions was not desirable for elevator service. So they do

not show really any economy; they were not commercial machines.

Mr. Sprague :—Let us examine some of the statements which have been made. , There have been six speakers.

Dr. Hutchinson has given some interesting figures bearing on the kilowatt hour expenditure per car mile of travel on the Postal plant between certain hours. These are interesting; but I think his conclusion that the average result would not be materially changed had the record been continued for a longer period of time is in error, as I will show later by some records of several thousand trips.

Mr. Hill has reproduced a good portion of my paper. I will later correct a number of his statements—some I shall ignore.

Another speaker, being somewhat timorous concerning electric elevators, advocates a mugwump system, a combination electric and hydraulic, having few of the good and most of the bad points of both. It might be pointed out that the ordinary motor pump and hydraulic elevator is such a system, but this is probably not the specific device which he has in mind.

Another sees great beauties in a type of machine which I am building, likewise the company which he represents, and exhibits a strange incapacity to determine differences between screw and drum machines which ought to be clearly manifest to any mechanical or electrical engineer.

Then again we have here the representative of the continuous operating system, whose advocate, contrary to one of his predecessors, believes in the supremacy of the electric elevator, so long as it is the particular type which he advocates.

Now, all these speakers are entitled, of course, to their respective views. It is true they are somewhat contradictory.

I will give a few facts now, and since the Postal Telegraph Building has been especially considered, I will take that plant.

First, I will state that there need be no mystery about the coal consumption. I have in my hand the operating log book handed me to-day by courtesy of the attorney of the building. The coal burned for all purposes from June, 1894, to December, 1895, averaged $6\frac{1}{8}$ net tons instead of $7\frac{3}{4}$. This is the official record, and not one quoted without authority.

The comparison of other buildings mentioned with the Postal is of little moment unless it takes into account the real duty performed, and just here I will point out the fact that it is a continuous service building, and more so than any other office building in the United States, except the Western Union, which gets a part or the whole of its steam supply from the street mains.

A cubic contents unit comparison is simply misleading. The lighting of the Postal alone is a large factor, and although there are three dynamos, each rated at 72 kilowatts, for the lighting, telegraph and ventilation, recourse is had sometimes to the street for the telegraph supply.

Averaged for six months, January to July, 1895, the engineer's record shows an *average* for the above service for 24 hours, and for every day, of 356 amperes at 115 volts, or 982 kilowatt hours per day, and to give an idea of how steadily this demand holds up at different seasons we may note the hourly averages for each month as follows:

January	387 amperes.
February	370 "
March	368 "
April	348 "
May	338 "
June	335 "
July	345 "
Average	356 amperes.

As distinguished from the other buildings mentioned, the Postal has in addition to the continuous lighting service, and an actual, not janitor, night elevator service, a club and kitchen supply, and a telegraph and a pneumatic dispatch service.

If the whole building, except heating, was supplied from a central station, the demands would be about as follows:

Lighting and ventilation	808	K. W. hours, or about	52 per cent.
Telegraph	174	" "	11 "
Passenger elevators	350	" "	23 "
Water pumping	25	" "	2 "
Miscellaneous	180	" "	12 "
	1537		100 per cent.

These figures should speak for themselves. Since the plant is actually run locally on $6\frac{1}{5}$ net tons of coal, then, assuming the coal distribution to be charged as per above percentages, 1.43 tons should be charged against elevators for each 24 hours, and it does its proportionate duty in heating.

It is, of course, difficult in a building which is always in use to make elaborate tests, but the following are submitted in addition to the above record:

The first is a coal record for the period beginning October 28th, 1894, and was made by Mr. Mackay's representative. The time was divided into three weekly parts. In the first week the entire service was supplied locally, in the second week the elevator service was supplied from the street, and the third week the entire service was again local.

Coal was measured by the number of loads of 2100 pounds each, and was as follows:

First week, 45 loads, local supply	94,500 lbs.
Third week, 48 " " "	100,800 "
Mean, first and third weeks, local supply	97,650 "
Second week, 43 loads, street supply	90,300 "
Average difference, one week, with and without local supply of elevator power	7,350 "
or 1050 pounds per day of 24 hours.	
Maximum difference, one week, with and without local supply of elevator power	10,500 "
or 1500 pounds per day of 24 hours	

The actual maximum record is about 50% of our estimate for the elevator service independently, that is, of 1.43 tons. This is probably in part because some live steam was used for heating the building, and because the boiler efficiency on a reduced power is a little less than when the elevators are running. It is interesting to compare this with the statement which was made by the Sprague Company as to what would be required in the matter of steam over and above the remaining service of a building in case electric elevators are used instead of hydraulics. There was made during the second week, November 3rd to November 12th, when the elevators were run from the street, 10,196 trips, a total travel of over 600 miles.

Our estimate was that the water evaporation would not exceed 250 pounds of water per car mile of travel. Six hundred miles would require an evaporation of not exceeding 150,000 pounds of water, or an average of 21,430 pounds per day. At eight pounds of water per pound of coal, which is a conservative rating of average boiler duty, this would be 2,680 pounds of coal per day as the maximum for this duty. Take another record, that of the Edison meter, for this same period from November 3d to 12th. This shows 3,108 H. P. hours charged. For 600 miles this would be almost exactly 5 H. P. hours per car mile of travel, or 3.80 kilowatt hours.

I see no reason to change my statement that on more modern plants the expenditure, averaged for all classes of service, will, with the average size of car, not exceed 3½ kilowatt hours per car mile.

On the night of November 22d, 1895, a test (to which expert engineers were invited), was made under the following conditions, in the presence of about twenty people:

All electric service, except elevators, shifted to street connection.

One boiler only left connected—B. & W.

All piping left in connection up to the engine and pump throttles.

Steam supplied to elevator engine, boiler and house pumps, and main pneumatic pump.

House pump prevented reliable use of water meter, but coal was accurately weighed, and nearly 2,000 electrical readings and the mileage of elevators were taken. One elevator was run on regular service, and five were run with 650 lbs. load (more than the average).

Boiler pressure	112 lbs.
Engine pressure	110 "
Pressure should have been	125
Exhaust into heating apparatus.	

Westinghouse direct equipment used.

RESULT:

Duration of test, 9.30 P.M. to 12.40 A.M., 3 hours, 10 minutes.

Number of round trips of 165 feet rise	744	
Total mileage	46½	miles.
Total coal burned	1163	lbs.
Less 10 per cent. for pneumatic and house service	116	"
Net coal for elevator engine	1047	lbs.
Coal per hour	331	"
Hourly mileage	14⅔	
Coal per car mile	22½	lbs.
Average kilowatts	54¼	
Kilowatt hours per car mile	3.77	
Water at 8 to 1 evaporation per car mile	180	lbs.
" at 9 to 1 per car mile	204	"
" at 50 lbs. per kilowatt hour per car mile	188½	"
Average indicated H. P. of engine	82	
" " H. P. per car mile of travel	6	

It will be noted that the electrical expenditures per car mile of travel taken under actual service conditions for our work in 1894 and that just given under a test at fixed average load are almost identical.

Let us take another record, one interesting also because of the diversity of opinion as to the average live load. One says it is one-half, another one-fifth. Which is right? I have in my hand the record of 328 trips, in which every man, woman and child that got into or out of two cars in a ten-story building, the Board of Trade of Chicago, was taken. I submit for record three sheets taken at random. (See pp 59, 60, 61).

The whole record is so long that I have not had time to average it, but it will bear out the statement which I have frequently made, that the average live load in a car does not usually exceed one-fifth of the normal maximum live load. In other words, if the car is rated at 2,500 pounds the average will not exceed 500 or 600. The meter record was taken of this same plant for the trips made, and shows an average of 4.1 kilowatt hours per car mile,—something over the Postal. Their cars are larger and heavier, and my guarantee was 4 kilowatts. Later it will run under the guarantee.

The Minneapolis plant I have never seen. The storage battery is being used because those locally in charge, at a time when I had nothing whatever to do with the installation of the plant, concluded that they would install a battery so that at certain hours of the day they could run their lighting and elevator service from one engine and the battery. I presume that is what they are going to do. The statement is made,—apparently gotten from newspaper clippings, that the machines at this plant are not up to guarantee. The test showed an apparent efficiency of 56 per cent., not 51 per cent., but it is probable that there was an error in the net weights reported, or some of the parts or the guides or sheaves may have been running hard, for they should have shown about 62 per cent. But this is not very important

PASSENGERS CARRIED.

Floor.	.....	1	2	3	4	5	6	7	8	9	Total In.	Total Out.	No. of Trip.
8.38 A.M.	Up.	11	....	....	1	....	....	....	...	...	12	....	28
	"	....	2	1	1	2	....	2	2	4	....	12	"
	Down.	....	..	....	....	....	....	....	....	....	0	....	"
	"	....	....	....	....	....	....	....	....	....	....	0	"
	Up.	10	....	....	....	....	....	....	....	....	10	....	29
	"	....	2	....	....	....	1	1	3	3	....	10	"
	Down.	....	....	1	....	....	....	....	....	....	1	....	"
	"	1	....	....	....	....	....	....	....	.	..	1	"
	Up.	7	....	....	1	....	....	..	....	...	8	....	30
	"	....	....	...	1	....	1	2	1	....	...	8	"
	Down.	....	....	....	....	....	1	....	....	....	1	...	"
	"	1	...	....	....	....	....	....	....	....	....	1	"
	Up.	3	1	....	....	....	....	....	....	....	4	..	31
	"	....	....	....	....	1	...	1	....	2	....	4	"
	Down.	....	....	....	1	....	....	1	....	....	2	....	"
	"	1	....	....	1	...	...	....	....	...	....	2	"
	Up.	8	....	....	...	1	...	....	...	....	9	....	32
	"	....	1	....	2	1	...	1	...	4	....	9	"
	Down.	....	....	....	4	....	...	....	1	....	5	....	"
	"	5	....	....	....	...	....	...	....	....	...	5	"
	Up.	11	1	....	...	....	....	....	....	...	12	....	33
	"	....	1	....	3	....	2	....	2	4	....	12	"
	Down.	....	....	...	...	....	....	1	1	3	5	....	"
	"	1	...	...	....	1	1	2	....	....	...	5	"
	Up.	4	....	....	...	...	...	...	....	....	4	...	34
	"	..	....	....	....	....	1	....	....	3	..	4	"
	Down.	....	....	..	...	....	....	....	...	....	0	....	"
	"	..	....	....	..	....	....	..	....	....	....	0	"
	Up.	10	1	....	...	....	....	.	....	..	11	....	35
	"	....	1	....	2	....	2	2	2	2	...	11	"
	Down.	....	....	...	....	4	....	..	....	..	4	...	"
	"	4	....	...	..	....	....	....	....	....	....	4	"
	Up.	6	....	...	....	..	....	...	....	....	6	....	36
	"	...	...	....	...	2	...	....	....	4	....	6	"
	Down.	...	2	3	3	1	4	....	1	1	15	...	"
	"	14	....	1	....	....	...	....	...	....	....	15	"
	Up.	8	2	...	....	....	....	..	....	....	10	...	37
	"	....	....	....	2	2	...	2	....	4	....	10	"
	Down.	....	....	1	....	1	...	....	....	5	7	...	"
	"	5	1	1	....	....	....	....	....	....	...	7	"
	Up.	12	..	....	....	....	....	....	....	....	12	....	38
	"	....	....	....	....	3	1	3	2	3	...	12	"
	Down.	....	...	...	2	....	....	2	1	....	5	...	"
	"	4	....	..	....	1	....	....	....	...	....	5	"
	Up.	8	3	....	....	....	...	....	....	....	11	...	39
	"	....	2	..	1	1	1	3	....	3	....	11	"
	Down.	....	...	....	4	2	....	2	...	3	11	....	"
	"	9	1	....	1	....	....	....	....	..	..	11	"
	Up.	12	2	....	1	....	....	...	...	....	15	....	40
	"	....	6	....	....	2	3	1	...	3	....	15	"
	Down.	....	...	...	...	...	....	1	....	.	1	....	"
	"	1	...	....	....	..	....	....	...	...	....	1	"
	Up.	7	...	....	....	...	....	....	....	....	7	....	41
	"	....	5	....	1	1	...	...	..	....	....	7	"
	Down.	....	....	....	....	....	1	....	...	1	2	...	"
	"	1	1	....	...	....	....	....	....	...	...	2	"
	Up.	3	3	....	....	...	....	....	....	...	6	...	170
	"	..	....	....	....	1	2	2	1	....	....	6	"
	Down.	....	...	2	...	...	....	1	1	....	4	....	"
	"	....	3	1	...	....	...	....	...	....	....	4	"
	Up.	1	....	...	..	....	...	....	...	....	1	..	171
	"	....	...	...	...	1	....	....	....	....	....	1	"
	Down.	....	....	....	...	1	2	....	....	....	3	...	"
	"	....	3	...	...	....	...	....	....	...	....	3	"
1.12 P.M.	Up.	5	...	.	....	....	.	....	....	...	5	....	172
	"	...	1	1	....	....	1	1	....	1	....	5	"
	Down.	...	....	....	1	....	..	...	1	....	2	..	"
	"	1	1	....	....	....	....	....	....	....	...	2	"
	Up.	2	10	...	....	....	....	....	1	....	13	...	173
	"	....	....	....	5	....	1	2	3	2	....	13	"
	Down.	...	....	....	1	2	....	....	....	....	3	...	"
	"	3	....	....	....	....	....	....	....	....	...	3	"
	Up.	7	9	1	....	1	....	....	....	....	18	....	174
	"	....	3	.	4	3	2	2	2	2	....	18	"

PASSENGERS CARRIED.—*Continued.*

Floor.	....	1	2	3	4	5	6	7	8	9	Total In.	Total Out.	No. of Trip.
	Down.	....	...	...	....	....	..	....	...	..	0	....	174
	“	....	....	....	...	...	..	....	....	....	....	0	“
	Up.	6	10	....	....	....	....	....	....	....	16	....	175
	“	....	3	....	4	3	3	1	1	1	...	16	“
	Down.	....	2	4	2	..	....	....	....	5	13	....	“
	“	10	3	...	....	...	....	....	..	....	....	13	“
	Up.	9	6	...	...	....	..	....	....	....	15	....	176
	“	..	..	....	4	2	1	4	2	2	....	15	“
	Down.	...	1	....	2	6	....	1	...	.	10	...	“
	“	7	3	....	....	....	....	...	....	..	....	10	“
	Up.	3	10	1	...	....	...	....	....	...	14	....	177
	“	....	....	...	4	6	2	1	1	....	....	14	“
	Down.	...	...	...	....	...	5	....	2	..	7	..	“
	“	4	3	....	...	....	....	...	....	....	....	7	“
	Up.	10	3	..	....	...	....	....	...	....	13	....	178
	“	....	2	....	....	4	....	3	3	1	....	13	“
	Down.	...	1	....	....	....	...	....	....	4	5	...	“
	“	2	2	....	....	....	1	....	...	..	....	5	“
	Up.	14	1	....	....	...	....	....	....	....	15	...	179
	“	....	....	....	1	4	3	2	2	1	....	15	“
	Down.	....	....	....	2	4	1	3	....	5	15	....	“
	“	9	4	2	....	....	....	....	....	....	....	15	“
	Up.	5	....	....	....	....	....	....	...	....	5	....	180
	“	...	...	...	....	3	....	1	1	....	....	5	“
	Down.	..	....	...	2	.	....	....	3	....	5	....	“
	“	3	....	....	....	....	2	....	....	....	...	5	“
	Up.	8	1	...	....	...	....	1	....	....	10	....	181
	“	....	2	....	....	2	2	2	1	1	...	10	“
	Down.	....	....	...	...	5	....	1	1	3	10	....	“
	“	7	2	....	....	....	1	....	....	....	....	10	“
	Up.	7	1	....	....	...	...	...	...	....	8	....	182
	“	....	1	....	1	....	1	2	1	2	....	8	“
	Down.	....	....	...	2	..	3	2	2	1	10	....	“
	“	5	4	....	..	1	....	....	...	....	....	10	“
	Up.	4	....	....	....	....	....	....	....	....	4	....	183
	“	....	1	2	....	..	1	....	...	..	....	4	“
	Down.	....	....	....	....	....	....	....	....	..	0	....	“
	“	...	...	....	....	...	...	....	...	....	....	0	“
	Up.	...	....	....	....	....	...	....	....	....	0	....	255
	“	....	....	....	....	....	....	....	...	....	...	0	“
	Down.	....	...	...	....	....	..	....	....	...	0	....	“
	“	....	...	....	...	...	....	...	....	...	....	0	“
	Up.	4	....	....	....	....	....	....	....	....	4	....	256
	“	....	1	....	...	....	....	1	2	....	----	4	“
	Down.	....	1	..	2	....	....	1	1	....	5	....	“
	“	5	....	....	....	....	....	....	....	...	....	5	“
	Up.	2	....	....	....	1	....	....	----	....	3	....	257
	“	....	....	...	1	...	1	...	....	1	...	3	“
	Down.	..	....	....	....	....	...	1	1	....	2	....	“
	“	2	....	...	....	....	....	....	...	..	...	2	“
	Up.	3	....	...	....	....	....	...	...	....	3	....	258
	“	....	....	....	2	....	....	1	....	....	...	3	“
	Down	....	....	..	....	....	...	1	....	....	1	....	“
	“	....	1	....	....	....	....	....	....	...	...	1	“
	Up.	2	1	....	....	....	....	....	....	....	3	....	259
	“	....	1	...	....	1	....	1	...	....	....	3	“
	Down.	....	....	....	2	...	2	1	1	...	6	....	“
	“	6	....	....	....	....	....	....	..	....	....	6	“
	Up.	2	....	....	...	...	.	....	....	....	2	....	260
	“	..	....	..	1	....	....	....	....	1	..	2	“
	Down.	....	....	....	....	1	2	1	....	1	5	....	“
	“	4	1	....	...	....	....	....	....	....	....	5	“
	Up.	4	....	....	....	....	..	....	....	....	4	....	261
	“	....	....	...	1	1	1	1	....	....	....	4	“
	Down.	....	....	...	...	....	1	...	....	2	3	....	“
	“	2	....	....	....	1	....	....	....	....	....	3	“
4.10 P.M.	Up	2	....	....	....	....	....	....	....	....	2	....	262
	“	....	....	....	....	....	1	....	1	..	..	2	“
	Down.	...	....	....	....	....	....	....	....	....	0	....	“
	“	....	....	....	....	....	....	....	....	....	....	0	“
	Up.	3	....	....	....	....	1	...	....	....	4	....	263
	“	..	....	....	...	....	....	2	2	...	...	4	“
	Down.	....	....	----	4	2	1	1	....	....	8	....	“
	“	7	1	....	....	....	....	....	....	...	....	8	“

PASSENGERS CARRIED.—*Continued.*

Floor.	.....	1	2	3	4	5	6	7	8	9	Total In.	Total Out.	No. of Trip.
	Up.	3	....	...	..	....	....	....	....	....	3	....	264
	"	....	1	...	....	2	....	....	....	....	....	3	"
	Down.	....	....	....	1	1	....	2	....	1	5	....	"
	"	4	1	....	....	....	....	...	....	....	....	5	"
	Up.	3	....	....	....	....	....	....	...	....	3	....	265
	"	...	....	1	1	....	...	....	....	1	....	3	"
	Down.	....	....	....	...	1	....	....	1	....	2	...	"
	"	2	....	....	....	....	....	....	....	....	....	2	"
	Up.	3	....	....	...	....	....	....	. .	....	3	...	266
	"	...	....	1	....	1	1	....	....	....	...	3	"
	Down.	...	....	....	....	...	....	. ..	2	...	2	....	"
	"	2	....	....	....	...	....	.. .	....	.....	....	2	"
	Up.	1	....	....	....	....	...	....	...	....	1	....	267
	"	...	...	. ..	1	....	....	....	....	....	....	1	"
	Down.	....	....	2	....	....	....	2	4	....	8	...	"
	"	7	1	....	...	...	....	..	....	....	. .	8	"
	Up.	3	....	....	1	1	....	....	....	....	5	....	268
	"	....	....	....	1	....	..	....	....	4	....	5	"
	Down.	....	....	....	1	....	2	1	1	....	5	....	"
	"	3	2	....	....	....	....	...	....	....	....	5	"

at the present juncture, for I hold in my hand the full copy of Prof. Shepardson's report, and I quote verbatim the closing paragraph as follows:

"The mèter was put in circuit Wednesday evening, January " 1st, 1896, at 6 P.M. Readings were taken each day at the same " hour. At 6 P.M. Saturday, January 18, 1896, the meter " showed that 1129.2 kilowatt hours had been used by the three " elevators in seventeen days. At the same rate, 1994 kilowatt " hours would be used in a month of thirty days. At the regular " motor rate of 7½ cents per kilowatt hour charged by the Minne- " apolis General Electric Company, this would come to $149.50 " per month. The discount of 35 per cent. for monthly bills be- " tween $100 and $150 would reduce this to $97.17 per month " for the three elevators for one month, or about $1.08 per ele- " vator per day. This remarkably low cost of power actually " used, shows the wisdom of your board in adopting electric " power for your elevators."

The plant is to be extended.

The Chicago Board of Trade plant is under the supervision of Mr. B. J. Arnold, a member of this INSTITUTE. The storage battery will be put in there because Mr. Arnold made the statement to this Board of Directors that with a single engine driving a 75 kilowatt dynamo he could run the four large elevators, a number of ventilating motors, and lights, both arc and incandescent, for a certain number of hours, and after that run entirely from the storage battery. It is not my province to criticise this plan or conclusion.

At my own works, I am about trying a storage battery, but not because we are in any danger of breaking down, especially as we have a duplicate equipment, but because I wish the facts.

Ground for criticism has been sought in the fact that the Postal has a street connection. That is a very sensible practice. They happen to have a good many motor converters which required absolute certainty of delivery of current for twenty-four hours for their telegraph service. Electric elevators at one time were not looked upon with general confidence, and there was opportunity for serious delays and troubles in starting so new a plant. Then, too, they wished to know if perchance it might be more economical to run from the street than from their own plant and as a wise provision, which people, under those circumstances would naturally make—it is now some two and a half years ago—they put in break-down switches connected to both the 240 and 120-volt circuits. I think that was an ordinary precaution.

A passing comment upon the report of the Edison company is not out of place. The screw machines, which are the specially characteristic machines built by the Sprague company, are generally operated from large isolated plants and not from the street, because individual elevator and light duty service has not been sought by it. That, of course, anyone can do without trouble My aim has been to surpass the hydraulic service in its highest type, which is almost invariably operated by private plants.

With regard to the criticism that it takes more current to start a gravity machine than an over-balanced machine, that goes without saying, but the reasons for using gravity control on the screw type of machine will be set forth later with reasonable fullness.

The statement that it requires 200% or 300% more to start a machine than to run it will not stand present investigation. In past work, there have been times when there has been from 100% to 200% excess of current used momentarily, not because the machine required that to start it, but because the resistance has been cut out of the circuit at a rate in excess of the building up of counter electromotive force. That objection has been done away with, and with the small amount of static friction it is possible to start a screw machine with a very small percentage above the actual running current, this depending upon the amount of counter-weighting carried, the speed at which a car is intended to be run, the load carried, and the rate of acceleration.

It is now perfectly possible automatically to control this starting current so that it will not be over 25% or 50% or any other percentage above the running current, and this method of operation does not interfere with the lights on a large central-station circuit.

We have gone so far even as to now run elevators during a good portion of the day on the same engine as the lights of the building, as in the Hotel Walton, Philadelphia, and this practice will grow rather than decrease, because of certain features which it is my privilege to introduce. I state without reservation that such potential variations as are given for the Postal plant do not

occur, nor can they occur, except under the most abnormal conditions and with engines and dynamos of the poorest character.

Mr. Hill quotes my statement that "measured by standard "practice, a given number of pounds of energy can be delivered "to the controlling apparatus of an electric elevator for less "pounds of steam, that is, water evaporated, through the medium "of no less than fifty combinations of engines and dynamos than "can be delivered to the valves of any hydraulic cylinder through "the standard pumps permissible in average elevator practice," and indulges in a curious mathematical flight wherein he gets lost in combinations and permutations. He says that this is a fair sample of the accuracy of the statements contained in my paper. I am glad to hear that frank acknowledgment, and it is fair to say that his criticism of it may be taken as the measure of his criticism of the balance. It would seem that my original statement was quite clear. Put in a different form, it is simply saying that there are no less than six or seven standard dynamos, and not less than seven or eight excellent engines, any one of which dynamos and engines can be employed, and by this combination energy can be created as stated.

Mr. Hill distorts this statement into the absolutely absurd conception that there may be fifty *transformations* of energy, and that a high percentage of return can still be obtained.

Now, as regards automatic control, I repeat, that in a hydraulic system the character of automatic control used on my electric elevators as it is practiced, is not feasible where a mechanical operating pilot exists, and if any device is applied to the pilot valves of the hydraulic elevator, to be effective in this way, it must be electric. Its use requires the operator to keep his hand on the lever under all conditions of car movement, and operates to cut the current off instantly in hoisting, or to bring the car to a prompt stop in lowering, in case the operator is negligent or is pushed away from his lever, or in case any accident happens to him. This control is not on other makes of machines of any kind or description, and in case of an emergency it is of the most vital character.

It is odd that some of the speakers have fallen into the mistake of assuming that we do not know exactly what our machines are doing, or what, with the improvements we are introducing, they can do. They also differ somewhat as to the relative costs of electric and hydraulic plants. My statement is entirely sound, that, measured by the prices which have obtained in this city for first-class hydraulic elevator plants, the electric equipments have been the less expensive plants to install, and even with the increase of generating plant are not necessarily more costly.

Generally speaking, on large loads the total efficiency of a screw machine will vary from 60 to 68 per cent., and on a drum machine from 40 to 68 per cent.

The best practice of governing over-balanced drum machines is

the use of a rheostat in the armature circuit up to one-half speed, and then the weakening of the field, exactly as I did on the elevated railroads some ten years ago.

I am not proceeding in ignorance of the physical laws covering the operation of electrical machinery. I try to meet all the conditions which arise in elevator service, not simply selected ones,—and no man in dealing with human life is justified in ignoring the abnormal conditions which may arise,—the failure of current, of a brake, a cutoff, breaking of a shunt field wire, slipping of a brake, running away of the machine, or the carelessness or ignorance of an engineer or an operator. All machinery, we may say, when operating normally operates with safety. What we are more concerned about is what will happen when conditions in an emergency are abnormal.

I have no apology to make for my strictures upon the drum machine, no matter by whom made. I say that up to certain speeds, and under certain conditions, it is the proper machine to use. Under certain other conditions and more extreme speeds it absolutely is *not* the proper machine, and I say further that any machine which has not got the power of self-excitation in case of emergency should not be tolerated on serious passenger work. I want to emphasize this fact just as plainly as I know how.

It is curious to hear the claim that a drum and a screw machine are practically the same, and it can only be accounted for by either a wilful disregard of facts, or because of ignorance of the essential principles of construction and control. I shall try to make the differences clear. There have been in the past two forms of hoisting machines,—one the steam worm gear drum elevator, the other the hydraulic, either vertical or horizontal. The hydraulic has had the preference over the drum machine in the matter of first-class service, and in point of safety.

Electric elevators are likewise of two types, the first being the drum—that is, the absolute counterpart of the steam drum machine, subject to its limitations and modified by the replacement of a steam engine by an electric motor. The second is the electric counterpart of the hydraulic macine, having all its "safeties," and something more than its safety. As I have stated, I build both types of machines, but I classify the duty to which, according to my judgment and experience, and that of engineers in general, they should be applied.

The drum machine is always subordinated in the class of work to which it is applied. It is not deemed by hydraulic companies nor by myself, the equivalent of the hydraulic elevator for first-class service, although the drum machine I am building has, for example, six times the gear surface under pressure of many machines. Its strength and efficiency in this way are materially increased. It has not, however, the elements of absolute safety which characterize the screw machine, and which will

relegate the hydraulic elevator to a subordinate position. In general:

The drum machine corresponds to the steam drum machine, long since given the second place.

The screw machine corresponds to the very best type of hydraulic elevator.

There are detail points of differences on these two types of machine,—differences which I consider absolutely essential on first-class passenger service:

1. The screw machine has absolute limits of travel.

The drum machine, on account of the endless character of its mechanical movement, has no fixed limits of travel.

It is therefore impossible to drive a screw machine into the overhead timbers, or send it into the basement by failure of the cut-off and brake. This has never happened to a screw machine. It can, and has happened to a drum machine.

2. The screw machine not only has the normal cut-offs and brakes, but, on the up movement, the mechanical part absolutely dislocates in case the cut-off fails to work, and it has, at the lower end, a buffer nut, which will stop a car going 600 feet a minute without danger, in case every automatic should absolutely fail.

The drum machine does not have, and cannot have, either one of these mechanical limit automatics.

3. The method of control on the screw machine makes it self-exciting, both in normal operation at its lower limit and in the event of failure of the main current.

The drum machine control generally adopted, that is, where over-balanced, is such that the machine cannot be self-exciting.

The power of self-excitation, the impossibility of demagnetizing the machine, and the fact that the current in the machine is never reversed, are essentials in the matter of safety under certain conditions of failure of current and brake, because the machine develops automatically a dynamic-brake of the most powerful character.

4. At least four hoisting ropes are used on a screw machine, and from five to six car and counter-weight ropes. The ropes at the hoisting machine have fixed leads, and are equalized on each side. The ropes on the car pass over one set of overhead sheaves, and each rope has equal duty under all conditions.

On a drum machine it is difficult to use more than two hoisting ropes, or at best three. They cannot be equalized at the machine. They have a shifting lead, and under certain conditions they are apt to jump the grooves. If the cars go to the basement, the back counter-weight ropes which must be there used are "broken-backed." There are three sets of overhead sheaves, and three different sets of ropes under unequal duty.

5. In addition to the "safeties" which characterize hydraulic chines, the screw machines carry a "centrifugal automatic" at the machine which prevents intentional racing of the car by a

careless operator, and can, if desired, make the machine a self-exciting dynamo on a closed circuit.

6. The screw machine can be built for any desired load or speed, for it is impossible to drive it past its automatics more than a fixed distance. It works on the gravity principle, precisely as the hydraulic does, and on that account has certain safeties not applicable to any other machine.

The drum machine should not be built for speeds higher than 300,′ because it has a carrying-by power on account of the greater masses in motion, rapidly increasing with speed, which render it liable to run into the overhead sheaves or into the basement in case of failure of cut off at a critical moment.

I hope I have made clear the differences in the two types of machines. Practical experience confirms my impression as to the importance of these essentials.

The tangible fact remains that, although electric elevators have been used for twelve years,—I put in one in the Pemberton Mills, Lawrence, Mass., in 1884,—it was not until the screw machine was developed that the hydraulic system was brought face to face with a real competitor. Its makers recognize that fact now.

Much has *not* been said by those who have been speaking upon the subject of hydraulic elevators, so I will make reference to two plants. The first is that of the Masonic Temple Building of Chicago, and it is interesting to note the opinion of the Chief Engineer of the Crane company, who, on the 10th of May, 1895, sent a report to the Vice-President of the Crane company, from which I quote verbatim as follows:

"Dear Sir:—

"Below you will find data as to the Hale elevators and "Worthington pumps as now operated in the Masonic Temple "Building, this city.

"The conditions under which the observations were made were "those in every day practice, no notice of preparation being given "that such observations were to be made.

"The observations began at 7 A. M., May 8th, and continued "uninterruptedly until 12.10 A. M., May 9th, so as to take in one "complete day's service of the elevator plant.

"Registers were placed upon the pumps so as to record each "stroke made, and watch was kept upon the elevators and the "trips made by all of the elevators during the day were noted. "The notes and figures give me the following results:

"The elevators consumed 612,166 gallons of water, and the "pumps delivered 1,469,722 gallons in the same time, showing a "discrepancy between the gallons of water supposed to be deliv-"ered by the pumps and the amount of water actually displaced "by the movement of the elevator pistons of 59%. In other "words, without taking into consideration whether the pumps "are the best that could be used for the purpose, or whether the

"elevators are the best that could be used, these figures establish "the fact that more than one-half of the coal now burned is ab-"solutely thrown away and is burned for no useful purpose "whatsoever, so far as running of the elevators is concerned."

This is followed by the usual modest claim that by a change in the hydraulic method of operation there can be a saving of nearly seven-eighths in the coal consumption.

Careful avoidance is made, I notice, of mention of the American Tract Society's plant, put into operation in May of last year, and supposed to represent the highest result of the combined skill and talent of the engineers of one of the most prominent elevator companies in this country,—a plant installed in full recognition of the fact that the hydraulic elevator, as commonly built, no matter whether for high or low pressure, uses identically the same amount of power for every foot of travel with or without a load. In this particular plant inverted differential piston cylinders are used, originally designed to work at about 750 pounds pressure, and to use water on one side of the piston when more than a certain load is carried, and on both sides on a differential plan on a light load.

The architects' requirements were that one car should make 700 ft. and five cars 600 ft. per minute, that they should make 1,200 round trips at full load in ten hours, and the guarantees by the hydraulic people were 1,750 lbs. of coal per day.

That building is reported to have practically abandoned the essential economic features of the plant. Pressures have been materially raised; the pumps and accumulators are disproportioned; a by-pass has been introduced in the high duty pump to prevent its sticking; the differential valve has been largely inoperative; the speeds guaranteed are not made; the loads called for are not lifted; the trips specified are not recorded. The building, while averaging for 24 hours less than one-half the lighting of the Postal Building, which has in addition its telegraph and pneumatic service and carries more people per day on its elevators, is burning not less than eight tons of coal per day, and it is stated that at times it goes as high as nineteen tons. I have not had an opportunity to verify the statement of the engineer, but I think these statements will be found to be pretty close.

Did time permit, I might add further pertinent reports, but these will suffice. I thank you, gentlemen, for your attention.

[Adjourned.]

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, March 25th, 1896.

The 104th meeting of the INSTITUTE was held this date at 12 West 31st Street, and was called to order by President Duncan at 8.15 P. M.

The Secretary read the following list of associate members elected and transferred at the meeting of Council in the afternoon.

Name.	Address.	Endorsed by
BETTS, PHILANDER 3d	Electrician, U. S. Navy Yard, Washington, D. C.	C. J. Field. Frank Bourne. O. G. Dodge.
BURGESS, CHAS. FRED'K.	Instructor in Electrical Engineering, University of Wisconsin, Madison, Wis.	D. C. Jackson. Wm. C. Burton. F. R. Jones.
CHAPMAN, A. WRIGHT	Electrical Engineer, Fort Wayne Electrical Corporation, Baltimore, Md.; residence, Brooklyn, N. Y.	Chas. S. Bradley. F. B. Crocker. M. I. Pupin.
FIRTH, WM. EDGAR	Chief Engineer, The Midvale Steel Co., Nicetown, Philadelphia; residence, 7203 Boyer St., Germantown, Pa.	Chas. B. Dudley. T. Carpenter Smith R. M. Hunter.
FORD, FRANK R. *M. E.*	Consulting Engineer, Ford and Bacon, 203 Broadway, New York City.	Jos. Wetzler. T. C. Martin. Wm. J. Hammer.
GIBBS, LUCIUS T.	Manager and Chief Engineer, Gibbs Electric Co., Milwaukee, Wis.	Maurice Coster. D. C. Jackson. H. J. Ryan.
GORRISSEN, CH.	Superintendent, Fort Wayne Electrical Corporation, Baltimore, Md.	Chas. S. Bradley. C. T. Hutchinson. F. J. Sprague.
GREEN, ELWIN CLINTON	Testing Department and Installing Work, Jenney Electric Motor Co. 206 South East St., Indianapolis, Ind.	Arthur Frantzen. H. H. Hornsby. C. C. Haskins.
HAMERSCHLAG, ARTHUR A.	Electrical Expert, and Owner, Hamerschlag & Co., 26 Liberty St., New York City.	O. P. Loomis. Jos. Wetzler. M. M. Mayer.

HULSE, WM. S.	Electrical Engineer, Fort Wayne Electrical Corporation, Baltimore, Md.	Chas S. Bradley. F. S. Hunting. W. M Stine.
LOZIER, ARTHUR DE LA M.	*M. E.* Salesman and Expert, Westinghouse, Church, Kerr & Co., 26 Cortlandt St.: residence. Hotel Winthrop, 125th Street, West, New York City.	E. J. Houston. A. E. Kennelly. R. T. Lozier.
MACKINTOSH, FRED'K.	Electrical Engineer, General Electric Co.; residence, 9 South Church St., Schenectady, N. Y.	A. L. Rohrer. H. G. Reist. W. L. R. Emmet.
MANSON, JAS. W.	Wire Chief. Franklin Street Exchange. Met. Tel. and Tel. Co.; residence, 80 York St., Jersey City, N. J.	J. J. Carty. H. Laws Webb. U. N. Bethell.
PHELPS, WM. J.	Electrical Engineer and Contractor, Chicago, Ill.	Alex Dow. Edw. Caldwell. Henry W. Blake.
PORTER, H. HOBART, JR.	Agent, Westinghouse Elec. & Mfg. Co., 120 Broadway, New York; residence, Lawrence, L. I.	C. T. Hutchinson. F. J. Sprague. W. D. Weaver.
ROSS, TAYLOR WILLIAM	Second Assistant Engineer, U. S. Revenue Cutter Service, Revenue Cutter "McLane," Key West, Fla.	H. J. Ryan. C. P. Matthews. C. H. Sharp.
SARGENT, HOWARD R.	Electrical Engineer, General Electric Co.; residence, 510 Union St., Schenectady, N. Y.	Chas. P. Steinmetz, John B. Blood. Wm. G. Ely, Jr.
STEWART, W. M.	Wire Chief, Met. Tel. & Tel Co., 18 Cortlandt Street, New York City; residence, 80 York Street, Jersey City, N. J.	J. J. Carty. H. Laws Webb. U. N. Bethell.
THURBER, HOWARD F.	General Superintendent, Met. Tel. & Tel. Co., 18 Cortlandt Street, New York City; residence, 49 Sidney Place, Brooklyn, N. Y.	J. J. Carty. H. Laws Webb. U. N. Bethell.

Total, 19.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by the Board of Examiners, Feb. 19th, 1896.

DOMMERQUE, F. J.	Chief Draughtsman, Chicago Telephone Co., Chicago, Ill.
HEATH, HARRY E.	Assistant Electrical Engineer, Eddy Electric Mfg. Co., Windsor Conn.
O'DEA, M. T.	Professor of Applied Electricity, University of Notre Dame, Notre Dame, Ind.
BADT, FRANCIS B.	Electrical Engineer, Siemens and Halske Electric Co. of America, Chicago, Ill.
BOILEAU, WILLARD E.	Superintendent and Electrician, Brush Electric Light and Power Co., Columbus, Ga.
BOYER, ELMER E.	Electrical Engineer, The General Electric Co., Lynn, Mass.

MITCHELL, JAMES,	Constructing Engineer and Agent, General Electric Co., Rio de Janeiro, Brazil.
COSTER, MAURICE,	Engineer, Westinghouse Electric and Mfg. Co, Chicago, Ill.
LLOYD, JOHN E.	Assistant Chief Engineer, Philadelphia Traction Co., Philadelphia, Pa.
POTTER, WM. B.	Engineer, Railway Dept. General Electric Co., Schenectady, N. Y.

The following paper was then read by Prof. Puffer, after which the meeting adjourned to Columbia University where the experiments were shown.

A paper presented at the 104th Meeting of the American Institute of Electrical Engineers, New York, March 25th, 1896. President Duncan in the Chair.

A NEW METHOD OF STUDYING THE LIGHT OF ALTERNATING ARC LAMPS.

BY WILLIAM L. PUFFER.

When a direct current of proper volume is caused to pass between the tips of suitable carbons, there is produced the phenomenon called the arc light. Generally the direction of the current flow is such that the upper carbon is the positive, and the lower the negative electrode. In order to produce a quiet and steady arc, the positive carbon may have a soft central core to aid in the formation of the crater from which the greater part of the light is emitted. The negative carbon may be cored or solid.

If we examine this arc light very carefully through dark glass, either smoked or colored, or better still, project an image of the arc on a white screen in a darkened room, using a good achromatic lens of large aperture and moderately long focus, we shall see pictured out a beautiful but inverted image of the arc in correct colors and intensities.

Knowing the direction of the current, we notice that the positive carbon has a very sharply defined crater of dazzling whiteness of about $\frac{1}{5}$ or $\frac{1}{4}$ the diameter of the carbon itself. From the crater the somewhat rounded point of the carbon diminishes in brightness, and finally verges into the dark unchanged carbon. Upon this heated carbon point the evidence of combustion is plainly seen in shape of changing surface and falling or accumulating ashes or scoriæ, while about it may be seen the flames of the burning carbon. The appearance of the negative carbon is quite different and clearly characteristic. The point is sharper, often with a little tip at the extreme end, while its base has a sort of crown or wall of ashes. The little tip is of whiteness equal

to that of the crater of the positive, but very small indeed. Below this tip is the hot pointed carbon which, however, is not nearly as hot as the positive.

In the space between the two carbons is the bluish violet light of the incandescent vapor in the path of the current. This is the arc itself, relatively non-luminous and in volume conical. The base of this blue cone is as large as, and is in contact with the crater, while the apex just touches the white tip of the negative carbon. Surrounding this core are several layers of gases of various shades of blue and yellow, and the flames of the burning carbons. The light thrown out by this combination of causes may be considered as coming from four sources. By far the greater part, from the white hot surface of the crater on the positive carbon, a much lesser amount from the little white tip of the negative; less than this from the carbons considered simply as two red hot sticks, and a very small amount from the hot gases. It is evident that a large part of the total light will be thrown downward.

Upon stopping the current, the heated carbons glow for some time: the positive is, however, very much hotter than the negative and keeps hot longer.

If, after the carbons have become cool, an alternating current is caused to pass between them, and sufficient time allowed for the points to become settled into their normal shape, it will be seen that the arc is naturally different in appearance from the direct current arc. Both carbons will be of the same shape, having rounded points, each with a small luminous crater, from which is emitted the greater part of the total light of the arc. Both carbons will show similar signs of combustion with the accompanying flames and ashes, and the light will be thrown upwards as well as downwards.

The blue arc between the two craters will appear as a band of nearly the same width in all parts, while about it are the several gaseous envelopes of different degrees of intensity. Upon shutting off the current, both carbons will be found of the same degree of luminosity and heat.

Somewhat more than a year ago, I wished to demonstrate to the senior class in electrical engineering during a lecture I gave them on alternating current phenomena, that, although when examined by means of a large projected image upon a white screen in a darkened room, the alternating current arc appeared to be

as steady as the direct current arc, and the light was thrown equally upwards as well as downwards, yet there were very great fluctuations in the light due to the intermittent heating of the carbon points.

The method chosen was the stroboscopic, which has given us at the Institute of Technology most excellent results in the hands of various experimenters while investigating the movement of transmitter electrodes, telephone diaphragms, tuning forks, vibrating strings, and the like.

A hand regulating arc lamp was placed in a lantern, with a large achromatic lens so adjusted as to make an image of the arc some ten feet long on the screen of the lecture room. A transfer switch and suitable rheostats were arranged so that either a direct current or an alternating current of about fifteen amperes could be used. At a convenient point a disk of about eighteen inches in diameter, in which were cut eight narrow radial slots, was placed so that the beam of light was interrupted as the disk revolved. The disk was fixed to the shaft of a direct current motor whose speed could be adjusted very closely by a rheostat in the armature circuit.

As the alternator used was a 125-cycle dynamo, and the disk had eight slots, the stroboscopic effect would be produced when the light of the arc would be allowed to pass at a frequency very slightly more or less than that of the dynamo, or $\frac{125 \times 60}{8} = 937\frac{1}{2}$ revolutions per minute of the motor armature.

Suppose, for example, it ran at 937 revolutions per minute, then, counting from the time when the current in the arc was zero, each receding flash of light would be $\frac{1}{250}$ part of a complete alternation behind the preceding one, and owing to the persistency of vision the arc would be seen on the screen, as if the alternations had been so reduced in speed that it took two seconds for a single alternation of the current.

An attentive watching of the image as the current alternates is now highly interesting and instructive, and can only be seen to be fully appreciated.

It is clearly shown that the alternating current arc is a sequence of direct current arcs, alternating in polarity, and that each wave of current produces very clearly and distinctly all the attributes of the direct current arc.

The hot positive carbon with its white hot crater, from which

extends the fan-shaped blue light of the arc to the small white tip of the colder negative carbon, will be seen to die away, and all light goes out except the glow of the red-hot carbons, and then light appears again with the current reversed.

Early in November last the subject was again taken up, with the very efficient aid of Mr. R. R. Lawrence, a post-graduate student in electrical engineering at the Massachusetts Institute of Technology, and rapidly developed with such beautiful results that it was decided to exhibit publicly before the the Society of Arts, which was done some little while after, at the regular meeting of January 2d, 1896.

We first attempted to take a set of instantaneous photographs of the arc at different periods of an alternation, and by the use of a pneumatic shutter, and a progressive motion of the lens, obtained some very sharply defined pictures. After many trials, this was given up, because of the practical impossibility of timing the exposures with respect to the alternations, and we decided to use a disk with half as many slots as there were pole-pieces on the dynamo, and to drive it by the shaft of the machine itself.

The dynamo available was one giving a three-phase 500-volt current with a frequency of about 60 cycles. Two wires only were used to give us the current required.

A somewhat long, light shaft, carrying at one end the disk, and at the other a positive mechanical clutch, was mounted in line with the armature shaft. As the clutch could only be thrown in when the two shafts were nearly equal in speed, a small motor was placed so as to bring the disk up to speed when the clutch was thrown in and the motor belt removed.

The disk was held in place by a frictional clamp disk on the shaft. A graduation and reference mark served to measure angular change of disk on shaft, and therefore of slots with reference to the pole pieces or alternation of the current.

The arc to be tested was put in a boxing to keep away air from the currents close behind the disk, and a camera with a roll holder in front of the disk. With this arrangement the arc as seen was perfectly steady at any part of the wave that corresponded to the position of the disk on the shaft, and as the process of stopping, setting and starting the disk was very rapid, the roll holder being in the meantime turned, many pictures could be taken in a very few minutes.

Generally it was not necessary to take more than twelve exposures in order to get a series showing clearly the changes in light intensities during a single phase.

We found that it was about as instructive to watch the appearance of the arc on the ground glass of the camera, and far more beautiful. In this way we examined both the effect produced in the arc by change in the voltage of the circuit, the current being kept constant by alteration of the resistance.

For example, with 500 volts, and a large non-inductive resistance in series with the arc, it was plainly evident that the current wave was approximately sinusoidal, as the time of extinction of the current, as indicated by the blue band of the arc proper, was very short, and the rise and fall of the current gradual and with no irregularities. This is to be expected, as the back E. M. F. of the arc is small compared to the voltage of the generator, and the circuit as a whole is non-inductive.

The opposite condition was realized by using a lower E. M. F. and regulating by a reactive coil. The time of no current was longer, and the current appeared to jump to its maximum in an exceedingly small angular time. In this case the arc was not steady, showing clearly to the eye that the succeeding waves of current were not alike either in form or current value, and also that the angle of lag was constantly changing. This fact has always prevented an accurate plotting of wave forms by the instantaneous contact method, and although known to exist, was never before actually seen.

A very pretty double arc was arranged by using three carbons and wiring two circuits, each with current regulators, in such a way that the arc was the common junction, and one carbon was of one polarity, while the other two were of opposite polarity. With wire resistances in each side, there was nothing peculiar to be noted other than the effects of the junction of two currents, but when the resistance in one circuit was gradually cut out, and equivalent inductance cut in, there was at once visible evidence of the lag of the current, together with the change of shape of the wave and the unsteadiness before noted. Owing to the long time of no current in the inductive side there were times when even with considerable lag there were actually no visible traces of current between either points. This effect was dependent on adjustment of current strength and inductance. as well as voltage. The sequence of currents and polarity in this arc was most beauti-

fully brought out when the disk was disengaged from the shaft, and driven by the little motor at a rate very slightly less than the dynamo.

We found that work in the immediate vicinity of the dynamo was not very desirable, owing to air currents and excessive vibration, so we arranged a combination of motors that will produce at any distance from the dynamo all the desired results.

A very nicely balanced brass disk with four radial slots in it was attached to the armature shaft of a Holtzer-Cabot synchronous induction motor of eight poles. The pulley of this motor could be driven by a light belt from a self-starting induction motor of the same make, which is, however, not quite synchronous under load. By trial the two pulleys are wound with rubber tape until their ratio is such that the brass disk will be uniformly driven at a speed a trifle above synchronism, and the arc light can be seen through the slots to pass through the alternation at a desirable rate.

By a single movement of a switch, the non-synchronous motor is cut out, and the synchronous cut into circuit when the armature drops into step with the dynamo, and the arc is instantly seen as fixed, the belt is thrown off or left on, as desired. These armatures may be on the same shaft, if necessary. The synchronous motor does not stand on its base, but rests on the turned-outside of its bearings in pillow blocks which are attached to a suitable base frame.

Concentric with the shaft and firmly attached to the motor is a brass gear, six inches in diameter, and on it a graduated circle. On a lever pivoted so as to be thrown to or from the motor gear is a small spur gear with a milled head for turning.

Turning this head will evidently cause the motor to slowly rotate about its axle, and as the armature must be in step with the dynamo, and as the turning of the motor changes the position of a given pole-piece relatively to the arc light, it follows that any part of the alternation of current in the arc may be seen on the screen, and as the motor has eight poles, a quarter turn or 90° on the graduated circle corresponds to a complete cycle in the arc.

The picture of the arc can then be photographed, measured, or in any way studied at leisure in any phase relation, as, for example, when the top carbon is positive, or when there is no arc at all and only dull red carbon points visible.

In this way we have seen single arcs of high and low E. M. F., long and short double arcs, arcs with much inductance in circuit, Jablochkoff candles, arc between a ring and a point within, the spinning arc between the ends of a carbon cylinder and a concentric carbon within, with a magnetizing coil around the inner carbon and the like.

One of the most beautiful arcs investigated by us visually and

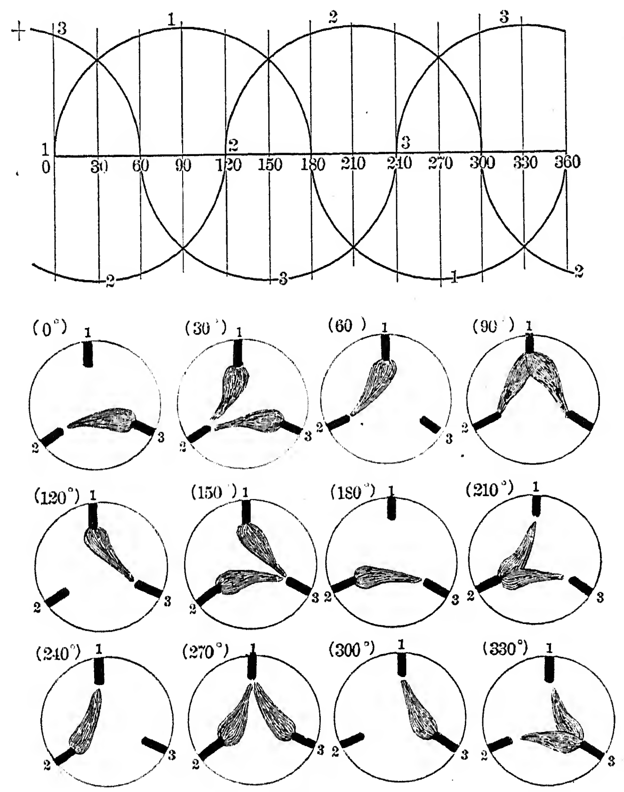


Fig. 1.

photographically was a rotary arc made by the use of three carbons in the same plane, at angles of 120° apart and wired up as the junction point of an external Y load on a 500-volt 60 cycle three-phase generator. Non-inductive resistance was used in the circuit, and the current used in one leg of the Y was 10 or 15 amperes.

Twelve photographs were taken at equal intervals of 30° in an alternation of the current in one wire. Fig. 1 shows very

clearly the relation of the current waves from the different carbon points, and the curved, fan-shaped figure indicates the position and direction of the bluish arc at the corresponding angle. The base of the fan rests on a positive carbon which has a white-hot crater and all the appearance of the positive carbon of a direct current arc, while the tip of the fan rests on the white spot at the end of a negative carbon.

It will be seen at 0°, for example, there is no current on carbon 1, and that 2 is negative and 3 positive, the blue fan-like arc curving from 3 to 2: 30° later, 2 is still negative and 3 positive, but that an equal arc is now playing from 1 to 2. At 60° 2 is still negative, 1 positive, but there is no current on 3. At 90° the appearance is somewhat like 30°, except that the signs are changed, and the point with the double current is necessarily much whiter, it being now positive. And so on through the changes of the complete wave.

This three-phase arc, when seen while the disk is running non-synchronously, is the most beautiful of any studied, and may be seen according to the different length of arc and the divergence of the disk from exact synchronism, either as a band of blue light which seems to be progressively travelling over the three sides of a triangular path, or as a rapidly spinning star of blue light, being in fact a rotary arc.

The three-phase arc is less noisy than the single phase, and its light is steadier and has less variation in its total intensity, owing to the fact that the current never stops, and there is always a positive carbon. Three cored carbons, placed parallel side by side, with slight magnetizing coils to keep the arc at the ends of the carbon, will give a very satisfactory light in the direction away from the tips, and may be used when it is desirable to throw the light all in one direction.

Four carbons at 90° apart, each with a suitable resistance in series with it, and connected to quarter-phase tap wires on a Gramme ring or other generator giving quarter-phase circuits, will also produce a rotary field arc of great beauty and interest.

Study of these arcs is still going on at the Institute of Technology under my immediate charge, which will, I hope, produce results sufficiently interesting to justify a second paper at some later date.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

MEETING OF WESTERN MEMBERS.

CHICAGO, March 25th, 1896.

A meeting of western members was held at the ARMOUR INSTITUTE, this date for the purpose of exchanging information in regard to the "Röntgen Ray."

About 250 members and guests were present, Vice President A. S. Hibbard presiding. Mr. Charles E. Scribner presented a historical sketch of the discovery, and also described experiments made by him and the apparatus used. Dr. James Burry also spoke on "The Surgical Value of the Röntgen Ray." Prof. Stine presented the following details of his experiments.

PROF. WILBUR M. STINE:—In the original paper by Prof. Röntgen the statement is made that when the cathode stream is deflected by a magnet, the X ray is given off from the new point of impact on the glass. By implication, it then seemed that the ray was produced at all points of the wall of the tube upon which the charged molecules constituting the cathode stream, impinged. This information proved too vague to materially assist the experimenter in investigations or in the design of Crooke's tubes. The fact that Geissler tubes do not generate the ray, indicated either that the ray was absorbed within the tubes, or that the cathode-repelled molecules lost their charge before reaching the walls of the tube. Again, were there two sources of the rays,—the surface of the cathode or some of the charged molecules of the stream, and the glass of the tube? Some time since, the writer showed that the fluorescence of the glass did not of itself, even when associated with a static charge, produce the ray. This pointed out the criterion for the successful tube, that it was one in which the vacuum was sufficiently high to permit a free molecular path from the cathode to the glass of the bulb.

Further, it has been recently stated on the best of authority that sciagraphs are produced not by the cathode, but are *anodic*. From the very first, the writer has kept these considerations in

view, and endeavored to obtain definite facts which should, beyond doubt, establish the source and distribution of the ray. In the following description the historical sequence will be preserved and a few leading dates indicated.

From the very first, penumbral effects were observed. The first accurate experiment was made about February 20. A rod of iron was separated a fixed distance from the plate, the distance of the plate from the end of the bulb being already noted. The penumbral effects were plotted back and showed that the rays were emitted from almost the entire end of the tube. This experiment was repeated and varied a great number of times with about the same results. Early in the present month a more exact result was obtained. Pieces of flat brass about $\frac{3}{4}$ inch in width were soldered together to form a geometrical figure. As these

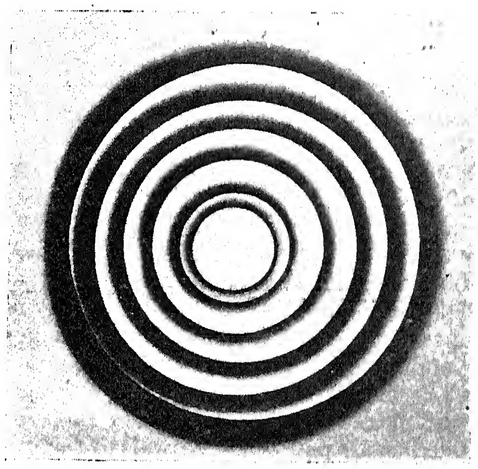


FIG. 1.

rested edgewise on the plate, the width of the shadows could be measured with great accuracy. These, when plotted back to the tube, indicated a source of the rays circular in shape and about $1\frac{1}{2}$ inches in diameter, this area corresponding in size and position with the dark ring, (the cathode imprint) on the bulb. Was this then, *one* or the *only* source of the ray? It then occured to the writer, that a short tube would indicate very clearly the space distribution of the rays. Accordingly, sections $\frac{1}{2}$ inch in height were cut from brass tubing, No. 18 gauge, of diameters ranging from one-half to three inches. These were placed concentrically on a dry plate, and a sciagraph taken with the rings parallel with the end of the bulb and at a fixed distance from it. The tube in this case was a small pear-shaped one. The shadows were concentric, and, plotted back to the tube, indicated that a circular

area about $1\frac{1}{2}$ inches in diameter was the prime source of the X ray (Fig. 1). To test whether the flat anode was also active, similar rings were placed opposite it. In this case, the shadows, instead of being concentric, were elliptical, and plotted back to the former area[1] (Fig. 2).[2]

Amongst the first experiments was one to test whether the ray could be polarized. It was found that tourmaline plates were sufficiently transparent. Two exposures were made, one in position of maximum transmission, the other at 90°, or in the crossed position. In both cases, with equal exposures, the tourmalines were equally transparent.

Another interesting experiment was to determine whether

A

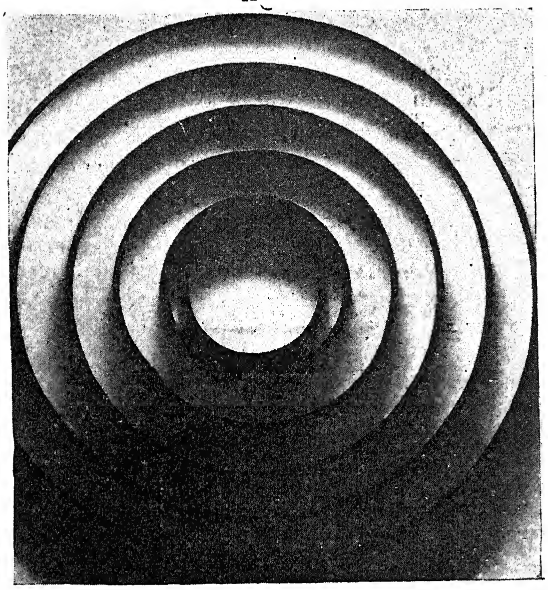


Fig. 2.

various grades of dry plates were more or less sensitive to the Röntgen ray. The results showed that the time of exposure was not influenced by the light-speed of the plates. Slow plates have since been exclusively employed, since they work with greater density and clearer shadows. The writer had the pleasure of being the first in this country to announce this important discovery.

This experiment possesses much theoretical interest. Röntgen and others have stated that the effect of this ray on silver salts was probably due to fluorescence which it induced on the film.

1. Shortly subsequent experiments made by the writer, employing the same method, clearly indicate that there is also a more or less weak secondary source, viz., wherever the cathode particles impinge upon the walls of the tube.

2. The letter "A" here designates towards bulb *opposite* the *cathode.*

Were this the case plates exposed to such rays would act in proportion to their light speed, since fluorescent light is highly actinic.

Much stress has also been placed on fluorescent glass bulbs as the source of the ray. To test this, a cube of uranium glass was powerfully excited by a focused arc light, but only negative results were obtained in repeated and lengthy exposures. The surface of the cube was also kept charged by a Holtz machine, but no results were obtained. The fluorescence of the bulb of the Crooke's tube seems rather an accidental than a causative phenomenon. The kinematics of the Röntgen ray was carefully and exhaustively studied. In short, no evidences were found of diffraction, refraction, reflection or interference. There are many appearances of such character, which have evidently misled some rather untrained experimenters, yet when carefully studied are found of negative value.

The writer's experiments have been extensive, and have been so fully described in the columns of the current technical papers that their repetition here seems unnecessary. Only a few of the more important results have been noted above.

Upon motion of Mr. B. J. Arnold a vote of thanks was extended to Mr. Scribner, Dr. Burry and Prof. Stine for their contributions upon the subject of the evening and the meeting adjourned.

DIED.

PECK:—En route from Mexico to Boston, June 13th, 1896, Samuel C. Peck, formerly of Boston. Mr Peck was born in Newtown, Conn., April, 1866, and was elected an associate member of the INSTITUTE September 6th, 1887, at which time he was electrician for the Simplex Electrical Co., at Newtonville, Mass. He subsequently entered the employ of the Thomson-Houston Electric Co., at Boston. In 1890 he went to Mexico, where he has been since continuously engaged as agent of the General Electric Co. He started North for the purpose of having a surgical operation performed and died on the train about three hours ride from the City of Mexico.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, April 22nd, 1896.

The 105th meeting of the INSTITUTE was held this date at 12 West 31st Street, and was called to order by President Duncan at 8.15 P. M.

The following associate members were elected by Council at the meeting in the afternoon.

Name.	Address.	Endorsed by.
BALDWIN, BERT L.	Mechanical and Electrical Engineer, The Cincinnati Street R'way Co., 72 Perin Building, Cincinnati, O.	Nelson W. Perry. L. G. Lilley. John A. Cabot.
CLARK, CHAS. M.	Student, Electrical Course, Columbia College; residence, 831 Madison Ave., New York City.	F. B. Crocker. Geo. F. Sever. W. H. Freedman.
CLARK, WILLIAM J.	General Manager, Railway Dept. General Electric Co., 44 Broad Street, New York City.	Louis Duncan. C. T. Hutchinson. Frank J. Sprague.
FIELD, HENRY GEORGE	Consulting Electrical Engineer, Field and Hinchman, 25 Hodges Building, Detroit, Mich.	Alex Dow. Jesse M. Smith. Henry S. Carhart.
FLORY, CURTIS B.	Student, Lehigh University; residence, 530 Broad Street, South Bethlehem, Pa.	Alex. Macfarlane. H. S. Webb. J. Henry Klinck.
GODDARD, CHRIS. M.	Secretary and Electrician, New England Insurance Exchange, Sec'y Underwriters' National Electric Ass'n, 55 Kilby Street, Boston, Mass.	F. E. Cabot. C. B. Burleigh. A. M. Schoen.
JACKSON, WM. STEELL	Electrical Engineering Student, Lehigh University, South Bethlehem, Pa.; residence, Duncannon, Pa.	Alex. Macfarlane. H. S. Webb. J. Henry Klinck.
LITTLE, C. W. G.	Engineer, British Thomson-Houston Co., 38 Parliament Street, London, Eng.	H. F. Parshall. Evan Parry. R. W. Pope.
McCLUER, CHAS. P.	District Inspector, So. Bell Tel. and Tel. Co., Richmond, Va.	M. B. Leonard. Geo. A. Tower. C. E. McCluer.

SPEED, BUCKNER	Asst. Electrical Engineer, Louisville Electric Light Co., 1521 4th St., Louisville. Ky.	G. Wilbur Hubley. L. B. Stillwell. Alex. J. Wurts.
THOMAS, ROBERT McK.	Asst. Chief Inspector, Bureau of Electrical Appliances, N. Y. Fire Dept.; residence, 222 West 23d St., New York City.	Joseph Sachs. Geo. A. Hamilton. H. F. Albright.
THRESHER, ALFRED A.	Electrical Engineer and Proprietor Thresher Electric Co., Dayton, O.	E. P. Roberts. R. H. Pierce. R. E. Richardson.

Total, 12.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners, Feb. 19th, 1896.

ANDREWS, W. S. — Electrical Engineer, etc., General Electric Co., Schenectady, N. Y.

Approved by Board of Examiners, March 20th, 1896.

STOTT, HENRY G. — Electrical Engineer, Buffalo General Electric Co., Buffalo, N. Y.

CARHART, HENRY S. — Professor of Physics, University of Michigan, Ann Arbor, Mich.

HARRISON, RUSSELL B. — President and Electrical Engineer, Terre Haute.

The following paper was then read by Mr. D. McFarlan Moore, and was illustrated with lantern slides. The hall was illuminated by the vacuum tubes described in the paper. Various experiments were also shown.

A paper presented at the 105th Meeting of the American Institute of Electrical Engineers, New York, April, 22d 1896. President Duncan in the Chair.

RECENT DEVELOPMENTS IN VACUUM TUBE LIGHTING.

BY D. MC FARLAN MOORE.

Most people have been accustomed to oil lamps, gas jets and other forms of light which have about reached their perfection. With the appearance of the arc and incandescent lamps it was thought that electricity had reached its limit in giving to the world a system of illumination that would leave nothing more to be desired. Indeed, it seems almost a presumption to dare to think of light being produced that would approach daylight in form and quality. The time is not so very remote when any man, who would have attempted to manufacture sunshine, would have shared the fate of a Galileo.

But fortunately the investigator of to-day has nothing of this kind to fear. Much arduous labor has already been expended in the solution of the problem by many eminent electrical scientists engaged in the study of vacuum tube phenomena, but the results from a practical point of view have been very meagre. This is chiefly owing to the complicated and expensive apparatus necessary, and the very unsatisfactory results even then obtainable. In fact, light from vacuum tubes, which is the only form of illumination that actually approaches nature's standard—daylight; has never been obtained in any quantity that would, in any way, be suitable for practical use. Of the other forms of electric lighting, the incandescent lamp is the most prominent. It is the peer of all illuminants in commercial use to-day, but is lacking, when we consider maximum uniformity in the distribution of light, and when calculations show that only three-tenths of one per cent. of the energy of the coal necessary to produce light by

incandescence (its name defines its character) is actually transformed into light, it is evident that there is room for improvement. The new electric light should possess all the good qualities of the present lamp with none of its drawbacks, and among its improvements will be noted the combination of utility and decoration. The recognized tendency of the day is towards multiplication of lights and avoidance of strong shadows—in other words, an even illumination, that is; light from all directions.

The object of this paper is not only to call attention to the advantages that will accrue with the adoption of vacuum tube lighting, but more particularly to a simple method of obtaining a current which will ultimately make such an adoption universally feasible. Almost without exception, experimenters in vacuum tube lighting have hitherto sought for the solution of the problem by merely pushing to the extreme, well-established methods based on principles long known in the art. That is, strictly speaking, no radical departures from the well-beaten paths have

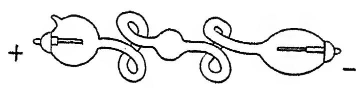


FIG. 1.

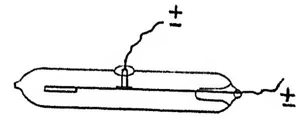


FIG. 2.

been as yet brought forward. However, this paper does represent radical departures: in principles, in apparatus and in the nature of the current, resulting in a light of greatly increased intensity.

Before entering upon a description of the new system, permit me to call your attention to the methods heretofore used for obtaining light from hermetically sealed glass tubes containing a rarefied gas.

For many years the Geissler tube has been a scientific toy. When a suitable electric current is connected to its terminals, its entire length is filled with a faint glow. This is, of course, a light of radically different character from that now used in any commercial form of illumination. (See Fig. 1.)

It is light emanating from rarefied air with an apparent absence of heat and combustion. Upon this principle developed, probably depends the light of the future, which will soon be, in the opinion of the writer, the "light of the present." As a device for transforming electrical energy into light, the vacuum

tube is very efficient. The majority of authorities place it at about 70 per cent. and the incandescent lamp at two per cent. Notwithstanding this remarkable efficiency, it has never been commercially possible to illuminate by vacuum tubes, because the light could not be made sufficiently intense (this is expressing it mildly) even with bulky apparatus that was entirely impracticable.

Furthermore, the current produced by such apparatus was of such a nature as to render its insulation extremely difficult. The ordinary induction coil is often used for this purpose. A current of low voltage, such as that from a battery of a few cells must be used with such a coil, because a current of higher voltage could not be properly disrupted, the arc forming, preventing a sudden break of the current. But since the light depends on the suddenness of the break, the arc must be prevented, therefore the quicker the break, the brighter the light—provided the apparatus is properly designed.

The quickest break can be made by interposing in a circuit the most perfect dielectric in the minimum space of time. The best dielectric known is a vacuum, and I have discovered methods for interposing it in rapid succession in a current in a minimum space of time, depending upon the principle of making and breaking a current rapidly in a vacuum.

The disruption of any current in the air results in the formation of a spark of greater or less length, and the greater its length, the less sudden the break. Therefore, if the break be made in a vacuum, the narrowest conceivable complete gap in the metallic conductor results in an almost instantaneous discontinuance of current, ensuring a maximum C. E. M. F. The current is thus interrupted in an almost infinitely short space of time as compared with all the ingenious mechanical contrivances, such as air-blasts and magnetic blow-outs devised for the purpose of breaking a current suddenly in the open air, but all of which are of little avail for the production of any quantity of light.

The vacuum vibrator, as shown in Fig. 2, is the nucleus of my invention. Although an exceedingly small device (not as large as one's finger) it demonstrates when in circuit with a small magnet (not as large as a tea cup), a principle embodying great possibilities. It is a new piece of apparatus, exemplifying a principle of value not only applicable in practical use, but also an improved implement for scientific investigation.

It is almost unnecessary to describe such a simple piece of apparatus, which consists merely of a spring rigidly supported at one end, and having attached to its free end a small disk of soft iron. A contact point rests against the spring at about its centre. A sealed glass tube, from which the air is exhausted, encloses both spring and contact point. The system, as a whole, is exceedingly simple. An electric current passes through a coil of wire and then through the vacuum vibrator. Wires in contact with the outside of each of the ends of a closed and empty glass tube are attached to the two ends of the coil of wire. This statement embodies the gist of the invention. (Fig. 3.) It will be noticed that this system is far simpler than the apparatus ordinarily used to excite Geissler tubes. The secondary coil is absent, reducing the expense and bulk many fold, as are also the metallic terminals sealed into the ends of the Geissler tube, but it produces light, the desideratum, in wonderfully increased volume.

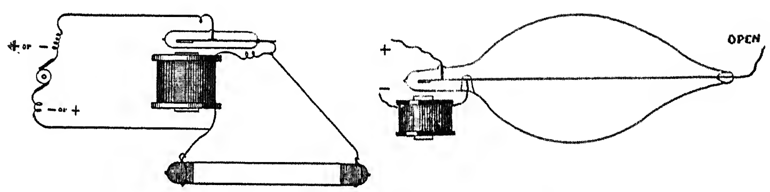


FIG. 3. FIG. 4.

With this apparatus, currents of almost any voltage can be rapidly and suddenly interrupted, and it is therefore now possible to obtain, by using ordinary commercial currents, strong light in vacuum tubes.

When the circuit through the magnet and vibrator is closed, the armature within the vacuum vibrates rapidly, disrupting the current within the vacuum at each vibration.

The resulting high-tension current excites a brilliant luminosity in another tube, usually of much larger dimensions, and containing a lower vacuum than the vibrator tube. There is, therefore, a necessity for two vacuums, one the very highest, the other very low. However, I have tried a number of experiments, using but one vacuum, practically amounting to an enlargement of the vibrator tube and a lowering of its vacuum. (See Fig. 4.)

This is manifestly not a good plan for the production of light, because the breaking of the current does not occur in a high vacuum, but it led to an interesting line of experiments, the most novel of which will now be brought to your attention.

Within this low vacuum is placed a wire which can be bent and shaped into any form desired, as shown in Fig. 5. When connected to the vibrator, the beautiful effect is immediately apparent, the wire being enveloped in a delicate purple glow. This can be applied to various purposes, such as advertising. One

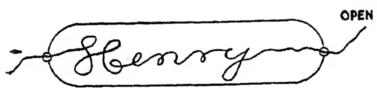


FIG. 5.

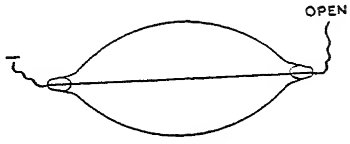


FIG. 6.

wire only, connects this sign tube to the vibrator, and it is attached to the armature terminal of the vibrator, because when so connected, it is the one which receives the high potential discharge of the magnet. In the bulb now shown (Fig. 6), the vacuum is higher, and a single wire extends through the centre. The light, instead of appearing as a purple envelope around the wire, now fills the entire chamber with a beautiful milky glow.

Close inspection, however, reveals the fact that there is a very small dark space immediately encircling the wire, and beyond it there appear to be rapidly moving rings of light, concentric with it. In fact, one is reminded of the field of force surrounding a conductor, as displayed by the familiar arrangement of iron filings—indeed, it is a similar phenomenon—the molecules of the residual gas taking the place of the iron filings.

The next bulb (Fig. 7) is similar to the one last shown, but with one exception. The wire is not single throughout its entire length; for a space of about three inches at its centre it separates into six strands, which thereby form a kind of cylindrical cage

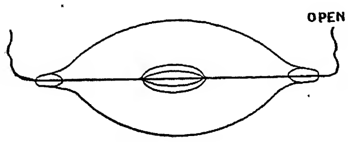


FIG. 7.

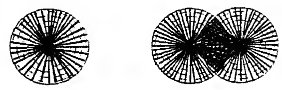


FIG. 8.

about one-half inch in diameter. In this case the entire bulb is not filled with a glow, but the interior of the cylinder forms a pencil of light quite dense, denoting that a new principle is brought into play. It is this—every current creates its own electrostatic field around its conductor, which, when immersed in a gas

at the proper degree of rarefaction, causes it to give forth light, which is most dense in a comparatively small circle surrounding the conductor. (See Fig. 8.)

When two wires, each having its field of force, are placed parallel to each other and about one-half inch apart, the density of the field between them will be doubled, and consequently the light in almost the same proportion. It will thus be clear that the pencil of light is due to the intersecting or overlapping of the fields of force of each of the six strands forming the cage. Upon this new principle many interesting lamps have been constructed, the problem being to get a maximum number of fields of force to intersect. Probably the best solution is in a cylinder made by spirally winding a wire, as in Fig. 9, causing its field to intersect in a manner that is almost ideal. This explanation may seem at variance with Faraday's famous experiment, proving that an electrostatic charge does not reside in the interior of the charged body. The vacuum may make the difference.

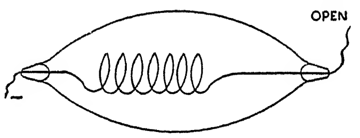


FIG. 9.

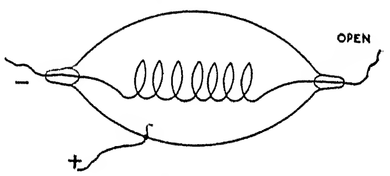


FIG. 10.

In this bulb (Fig. 10) there are two separate terminals projecting from the glass. When the spiral is connected to the negative pole of the vibrator, and the other terminal is made positive, the light is greatly increased. Such a lamp can be very conveniently made by using an incandescent lamp bulb, as in Fig. 11. The bit of platinum wire extending into the bulb and forming the positive pole, can be placed in any of the positions 1, 2, 3, 4, 5, without affecting the light in the spiral, but it is apt to become heated, and this is remedied by attaching to it a metal wire ring. (See Fig. 12.)

In these lamps a large proportion of the light is confined within the spiral, and since volume is desirable, the idea of increasing the number of spirals suggests itself, as in Fig. 13. But the total volume of light emitted by four spirals is only equal to that of a single spiral lamp, that is, each pencil is but one-fourth as bright; hence, to bring them all to full brightness, the energy of the inductive current should be increased in proportion.

With the idea of obtaining from the entire bulb a uniform glow, the lamp shown in Fig. 14 was constructed. The "filament" consists of a great many complete loops of very fine aluminium wire. Fine wire was used, not only because of appearance and weight, but also for two other reasons: 1st, because a fine wire has about as large a field of force as a much larger wire; and, 2nd, because it does not obstruct the light so much, and at the same time is a minimum of metal within the vacuum. This is a matter of much importance, as upon it largely depends the life of the lamp. A great many different conductors were used for the construction of these filaments, the main idea being to use that material which contained the least occluded gas, and would be disintegrated a minimum by the action of the current. Fig. 15 shows ordinary incandescent lamp filaments utilized in a glow lamp, but one leg only is cemented at the centre of the bulb to

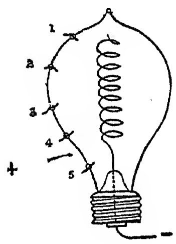


FIG. 11.

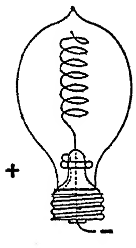


FIG. 12.

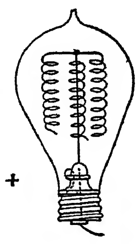


FIG. 13.

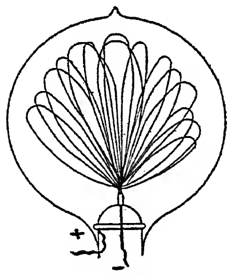


FIG. 14.

the negative terminal, the other being free—a veritable fountain of filaments. A rather curious and not entirely explained phenomenon was noticed in this form of lamp, *viz.*, the free ends of the filaments were apt to be violently agitated, and whenever they touched the glass of the bulb, they heated up to a bright red throughout their entire length, producing a most brilliant combination glow and incandescent light. Many are the advantages of these lamps over those using fluorescent materials, as the sulphide of zinc or calcium. Sometimes when the exhaustion is carried a little too far, the vibrator current is unable to affect the lamps, but after they are held in contact with one pole of a large induction coil for a few moments, and then connected to the vibrator current, the trouble ceases.

In all of these forms of lamps it is very interesting to note that in order to get maximum light, the sub-divided terminal of the

lamp must be so connected to the mains that it is negative. This is interesting to remember when the subject of lighting tubes is considered.

The class of lamps will now be considered where, instead of sub-dividing the lighting electrode into filaments, plane surfaces are used.

The bulb in Fig. 16 contains two pieces of sheet aluminium equal in size, set with their planes at right angles to each other, in order that a minimum of light may be interrupted from any point of view, and that the positive will act as a reflector to the negative. These pieces of aluminium must be carefully cleaned before being placed in the lamp, because any grease upon them will cause beautiful tufts of light all over their surfaces, instead of a glow filling the bulb, and the vacuum will soon be lost. Of course, in a lamp of this construction the poles can be changed

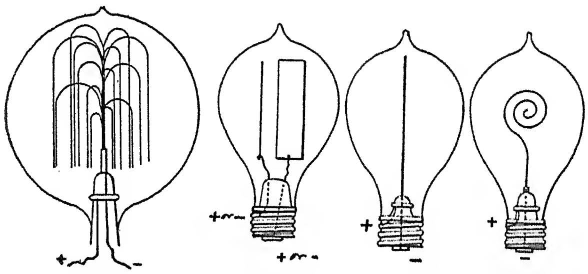


FIG. 15. FIG. 16. FIG. 17. FIG. 18.

with impunity, and if an alternating current be passed through the vibrator, both plates will give light, but the total amount will be the same as when direct current is used.

One of the many modifications of this form of lamp was to make the magnetic pole in the form of a small cylinder of aluminium gauze.

Many lamps were made on the principle of a spiral within a spiral, wound in the same or opposite directions, and also of using metallic coatings on the tubes interior and exterior, as well as using bulbs of all shapes and sizes. One important advantage to be noticed in all these forms of lamps is the total absence of the very objectionable striations, such as occur in the ordinary Geissler tube.

Fig. 17 is a very simple form of lamp—merely a single piece of straight carbon filament producing the light.

Fig. 18 is another of peculiar form—an unusual density of white light inside the convolutions of the spiral.

To determine whether there was any appreciable heat at the centre of an intense pencil of light, a number of lamps were constructed in which various substances were placed at the point of greatest density of light, as in Fig. 19, but in no instance was the substance affected in any degree whatever.

Following out this idea a little further, there was inserted a small glass tube containing air properly rarefied in the centre of a spiral, as in Fig. 20.

But the inside of the small tube remained dark; nevertheless, the glow outside of the spiral, dark space and other phenomena, even to the tufts of light between some of the convolutions of the spiral are the same as those in a similar lamp without a tube, inside its spiral.

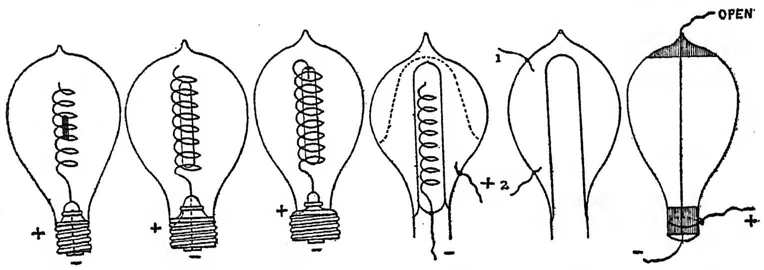


FIG. 19. FIG. 20. FIG. 21. FIG. 22. FIG. 23. FIG. 24.

Another lamp was then made the same as the last, except the tube inside the spiral has a platinum wire extending into it, which wire is electrically connected with the spiral (Fig. 21.) Of course, the small tube was tried separately before being placed in the lamp bulb, and it filled with a white glow. But after the lamp was completed it refused to give any light; the glow outside the spiral, however, was the same as though there was no inner tube present.

Hoping to get a lamp with almost no metal in the light-producing vacuum, the lamp shown in Fig. 22 was constructed—the spiral is within a separate tube. When tried, a tuft of light appeared in the top and bottom of the small tube which was surrounded by a faint glow, the outlines of which are shown by the dotted line.

The next step was to do away with the inner vacuum and con-

struct a bulb as shown in Fig. 23. When a spiral was inserted in the tube and connected to the negative pole, the positive being either 1 or 2, a dark space one-eighth inch deep surrounds the tube, beyond which a faint pinkish glow appeared filling the bulb. The phenomena known as "afterglow," which is sometimes noticed in evacuated bulbs after having been subjected to an electrostatic strain, I have been able to obtain, but very seldom. (Fig. 24.) However, this bulb could be picked up and carried around the room, but every time it was picked up after being laid down, a discharge resulted, which, being repeated three or four times, dissipated the glow entirely.

From a tube containing two parallel wires, shown in Fig. 25, but with a low degree of exhaustion, the glow was entirely absent, but instead, brilliant, flaming yellow discharges completely filled the space between the wires, which was over half an inch wide and about two feet long.

At the beginning of the lecture your attention was called to

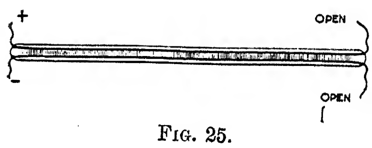


FIG. 25.

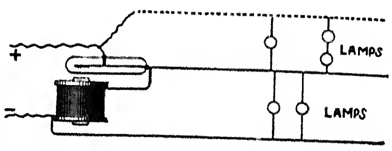


FIG. 26.

the great similarity existing between the field of force of a "cold-light" giving conductor in a partial vacuum, and that of a conductor conveying a current in the open air. Care, however, should be taken to note that the light from the rarefied atmosphere is due to electrostatic and not electromagnetic phenomena.

For example, I have here a powerful electro-magnet which I connect directly across the lighting mains, and therefore, it is one at whose centre or core the electro-magnetic field is very dense. When this tube, which has a diameter the same as that of the core of the magnet, has about half of its length passed through the centre of the magnet, no light whatever results. If, however, but one terminal (the other being free) of this same magnet be connected to the vibrator, immediately the tube gives forth light.

Referring again to the subject of connections, Fig. 26 shows the light connected around the terminals of the magnet; it also can be connected with equal results around the break or spark

gap as shown by the dotted lines. Your attention is called to the fact that lamps of this kind will operate equally well, whether connected in multiple or in series, provided the area of the negative electrode is about the same in each ; if not, that one in which the negative electrode is of greatest area will alone light up.

If a single bulb of small size be connected to a circuit of considerable induction, a well-defined discharge is liable to occur which will ruin the lamp.

In Fig. 27 an inductive resistance is distributed with each lamp, making the system of distribution self-regulating, that is, the turning on or off of lamps will not affect the brilliancy of those burning steadily.

Referring again to the diagram of circuits showing the system in its simplest form: If good results are to be obtained, the magnet must be designed and constructed with the greatest care. Its duties are two-fold; first, to give the vibrator its mechanical motion, and second, to act as an inductive resistance. The iron core must be proportioned to the conditions of the circuit. If there be too much or too little, the light suffers, but a certain amount of iron should remain, in order that the magnet have sufficient power to vibrate the armature. Similarly if there be too many or too few turns of wire on the magnet, the light is decreased. From this it is evident that vibrators of different rates will not be suitable to the same magnet. However, even when a vibrator is connected to a suitable magnet and circuit, and produces a good light, it can be further improved by "tuning the circuit," that is, altering its self-induction by varying the amount of iron in the magnet's core. It should also be stated that by this means a maximum light is obtained from a minimum current. In order that the time constant of the magnet be a minimum, and to ensure rapid action, the magnets should be short and thick. Large induction coils cause the tubes or lamps to give forth but little light, whlle a small magnet, whose length of wire is not $\frac{1}{100}$ that of the induction coil, will cause the tubes and lamps to light up brilliantly.

It is interesting to note that with a comparatively few turns of wire, a very small one-volt battery will give quite a strong glow. This glow can be intensified by using a secondary coil.

Although the various lamps that have been described are of great interest, nevertheless the very nature of lighting by luminescent gas is such, that it is far more applicable to radiate from

sources of considerable area than from units of light of small area. The best method of obtaining light from a large area is by the utilization of tubes of considerable length, instead of small bulbs. The light of these tubes should be entirely free from the very objectionable striations always present when interior electrodes are used; but these striations are entirely obviated by using exterior electrodes. They may be metal caps on the outside of the tube, or preferably merely coatings of metallic paint. Such tubes can be made up in almost any lengths, and can be bent into a great variety of forms, making them suitable for exquisite decorative effects. Under this head can be mentioned the fashioning of tubes in the form of letters, which may be used as electric signs.

Permit me to call your attention once more to the key of the whole system, viz.:—repeated interruptions of an electric current in a high vacuum.

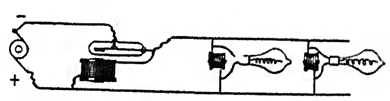
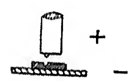


Fig. 27.

Fig. 28.—Electric Light Carbon.



Vibrator Contact Points.

The simplest method of accomplishing this object is to hermetically seal within a glass tube a vibrator of ordinary form, but its exact construction to give the best results has been a matter of tedious experimentation and study. The very slightest alteration in the dimensions of almost any of its parts—such as the length, width and thickness of the spring, or its method of mounting, or the position of the contact points, or the thickness or diameter of the armature, will cause it to be a very good, or a very poor vibrator. Again, the operations of the glass-blower had to be watched most carefully. Only certain kinds of iron and steel were selected, to avoid occluded gases, and even then they must undergo a special treatment before being fit for use. The selection of suitable contact points has also been a large field for research. Nearly all known conductors have been tried, and many interesting facts have developed in this connection, not only so far as the direct action on the various metals in vacua and various gases is concerned, (and this several in instances is

the reverse of the phenomena noted in open air), but also with reference to the electro-deposition or electrolytic action that takes place. For instance, as is well known, the positive electrode is the one which disintegrates most rapidly in the open air, and its apex is usually concave. This is probably best shown in the ordinary direct current arc lamps. If aluminium or any soft metal of comparatively low fusing point be used as contacts in a vibrator, after about a day's run, an examination shows that the shape and condition of the contacts is just the reverse of the way they appear after use in the open air. That is, the positive terminal looks like the negative, and the negative like the positive. (See Fig. 28.)

I have constructed several dozen distinct varieties or amplifications of the ordinary type of vibrator, such as multiple contacts, etc.

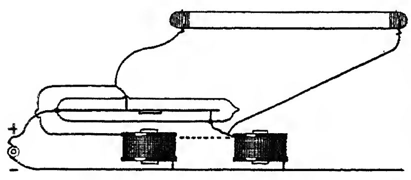


FIG. 29.

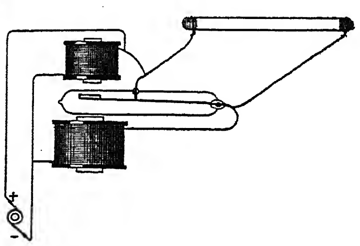


FIG. 30.

Fig. 29 shows one form, in which the object was to produce a larger number of breaks of the current per unit of time by providing two contacts, as shown. In some respects this scheme worked well, that is, the light was stronger than that produced by an ordinary vibrator with a single magnet, but the light was not steady, and when the connections were changed so that but one magnet was used, the light flickered still more. All the vibrators so far considered, have had their springs so adjusted, that the contacts remained normally closed. Fig. 30 shows a circuit using a vibrator normally open. The small magnet constantly closes the circuit, and the large one opens it. This arrangement gave a good light. A number of ordinary vibrators have been operated, arranged with the magnet above the armature, so that gravity helps close the circuit. Of course, springs and armatures of scores of shapes and sizes, including gravity vibrators, have been tried, the principal object being to obtain a method

for taking up the wear of the contact points; but the best solution of this problem lay in getting contact points that would not wear because of a more perfect vacuum.

Turning now from the mechanical construction of vacuum vibrators to consideration of their unique properties from a scientific standpoint. As stated before, roughly speaking, the prime object of making the break of the current in a vacuum is to produce an increased C. E. M. F. over that resulting from an air-break. But I venture to say, few as yet realize the possibilities and the tremendous advantages involved in the separation of electrodes in high vacua at regular periods of time. It is indeed questionable whether the increased C. E. M. F. of the ether gap is responsible for the results. It is more probably due to the fact that the wave lengths of the current resulting from an ether gap differ radically from any that have heretofore been known of. Recognition of the importance of this phenomenon led me to use the word "etheric" in connection with my system of tube lighting, because the light is directly dependent upon what I designate as the ether gap of the vacuum. Our best notion of the separate existence of the ether is formed by thinking of that which remains within a vacuum of the highest degree we can produce. It may have been noticed, that so far I have not used the word "phosphorescent." It is undoubtedly a misnomer as applied to tube lighting; but to make matters worse, there seems to be no word in existence exactly or even approximately suitable.

The word etheric is especially applicable to light produced by the ether gap. That incomparably better results should be obtained by using a vacuum as a dielectric seems to be in perfect accord with accepted theory which has largely been upheld by actual experimentation. For example, setting aside for the time being that part taken by the magnet, let us consider only the break,—the spark.

First, what is required is a continuance of the rapid oscillations of an electric discharge, and I desire to show that this is accomplished by a vacuum vibrator in an ideal manner. One method of obtaining an oscillating discharge is to use a potential such that an air-gap of considerable width is bridged by the spark. But the manner in which such discharges succeed each other largely depends upon the irregular movement of the air within the gap.

This method is unsatisfactory for many other reasons, among them the very objectionable high tension required, but there remains the conductive method of causing a spark, exemplified in the ordinary vibrator and induction coil. Here the sparks in the primary circuit can be made to succeed each other quite rapidly, but the length of time required for each complete break is long. However, with the vacuum vibrator, a new and simple device, the conditions are different, and so are the results. Here the dielectric is an exceedingly thin film of ether, which is capable of withstanding a great electrostatic strain, but when it does break down, it does so very suddenly; that is, it may be considered a *perfectly* disruptive discharge, and therefore its single oscillations are very short; but as a whole they are long continued. This means that the frequency is high, and, according to Maxwell, very high frequency oscillations are probably identical with light.

The small magnet, being a circuit of induction, has its own natural period. But its period and moment of inertia will be less, the less its capacity. That is, the smaller the magnet the smaller the period, or the higher its frequency can be.

But to have an intense light, there is required a high E.M.F. of self-induction. This the small magnet furnishes, because what is lost in self-induction by using a comparatively few turns of wire is more than made up by the suddenness of the discontinuance of current flow, and the fact that the high vacuum precludes the possibility of loss to the self-induction ordinarily due to the glow discharge which precedes the disruptive discharge in the air. The self-induction also depends on the *amount* of current flowing through a circuit when interrupted, and this the vibrator provides for, in that *all* the current which flows through the coil does not pass through an arc, but is transmitted over actual metal contacts. Also since these contacts are so very close, due to the thinness of the dielectric, the oscillatory discharges do not leave the metal and pass in objectionable minute streams through the vacuum. And the number of oscillations in the coil and the amount of light, depend in a measure on energy expended to overcome the resistance of the dielectric which is almost infinitely greater than that of air.

That the silent discharge prevents long continuance of oscillation is shown in the Hertz experiments, where the experiment fails unless the balls of the electrodes are kept polished. That

the vacuum vibrator is nearly ideal, is again shown in that these troubles are almost entirely eliminated. The ether is undoubtedly the ideal medium in which to disrupt an inductive circuit for conversion into light. Since an exceedingly thin film (if this term may be so used) is a dielectric of such strength that a very small displacement results when it is disrupted, and the ether being the medium of minimum rigidity closes the "hole" the instant it is pierced. Such a medium for such a purpose is almost incomparable to air or oil, which becomes volatilized.

In order to get any oscillations, more sudden rushes of current must occur on discharging than on charging, and the more nearly these equal each other, the quicker the rushes will succeed each other. Now, in the case in question, when the tube circuit with its condenser coatings has a certain capacity, the self-induction of the magnet can be so varied that these two will always neutralize each other, and then the critical strain on the dielectric requires but little energy to cause a discharge, and the circuit being almost balanced, the surges follow each other in rapid succession through the tube. The surges continue for a longer time, since the energy of the discharge is not dissipated in heat on the air, but is conserved to be utilized in prolonging the existence of the oscillations. The use of this vibrator seems to afford the best means yet invented for impressing molecular disturbances in a tube. The longer the oscillations exist, the more nearly the mean oscillation approaches a constant period, and this period is practically governable as compared with that of currents due to magnetic blasts or heated air currents. The higher the frequency, the greater the mean free paths of the molecules, because a less number of molecules will then be required to cause a given number of collisions, and the less the number of impacts, the greater will be the light in proportion to the heat. But the frequency is dependent on the capacity of the condenser, which, in the case of the tube coatings, is very small.

When the self-induction and capacity are properly proportioned, and the rate of the vibrator is the same as the natural period of the coil, *or any of its harmonics*, then there is resonance, and the maximum amplitude of each impulse will be constant. In this case the current flow is at its maximum, but so also is the voltage at the ends of the tube, as well as the quantity of light produced with a minimum expenditure of energy. The velocity of the lines of force produced by an oscillatory discharge is supposed

to be equal to that of light, but its wave lengths are exceedingly greater. But this disparity is greatly reduced by the vacuum vibrator. This accounts for the intense light in the tube at comparatively low potentials, and indicates that nature's keyboard has been struck to the tune of 500 trillions of waves per second, and that this rate is maintained by the fundamental mechanical vibrations of the vibrator—about 100 per second—a most beautiful demonstration of nature's wonderful compass. But not only is more light produced by the vacuum vibrator than was heretofore obtainable, but there accompanies it many other advantages of particular importance. Three of these can be mentioned:—first, simplicity and greatly reduced cost of apparatus; second, the obviation of impracticable potential, and third, a very marked advance in economical production. The first heading, simplicity, has already been dwelt upon. Compare an inexpensive magnet, not as large as one's hand, and a vibrator the size of one's finger, attached to commercial currents; with apparatus costing thousands of dollars, consisting of a high speed alternating dynamo of many coils, oil transformers, disruptive discharges with magnetic blast, induction coils and condensers. The many seemingly insurmountable difficulties encountered with this method, are almost completely overcome by the simple expedient of the ether gap. Or, referring to ordinary induction coils, the vacuum break affords a means for obtaining from the few turns of comparatively coarse wire, results not obtainable with mammoth and expensive coils, made of many miles of wire, and capable of creating enormous differences of potential.

The second heading is essentially a practical one. It has often been argued, to the detriment of tube lighting, that since it was admitted by its supporters that enormously high potentials were absolutely requisite to cause any appreciable amount of light, that therefore (and the argument was logical) the whole idea was extremely impracticable unless some new insulator be discovered, that could cope with the high potentials, so difficult, dangerous and expensive of generation and manipulation, as to prohibit their use commercially. But with a current endowed with such properties as are given it by a vacuum tube vibrator, no new insulator is needed.

A light now results many times brighter than that formerly due to millions of volts, able to pierce several inches of hard rubber, or produce a spark many inches in length, from a current

transmitted to the bulb or tube over ordinary flexible cord, and whose sparking distance is less than $\frac{1}{16}$ of an inch. Neither can any shock be felt from such a current. Another example of the comparative ease with which this new current can be insulated is apparent in the magnets, wound in the ordinary manner, in striking contrast with the necessity for expensive and cumbersome oil transformers. In this light the exclusion of all gaseous matter does not seem to be a matter of such vital importance.

Closely allied with the subject of insulation is that of frequency. The higher the frequency, the lower the potential can be, not only with respect to light, but also to insulation, because irregularity in the rate of vibration puts the insulation to a severe test. However, the period of the vibrator is not rapid as compared to that of alternating dynamos, constructed to obtain similar lighting effects, but resulting in those of lesser degree. The alternations of such a machine are about 30,000 per second, which is further increased by a disruptive-discharge coil. This was necessary to compensate for the long wave lengths of the current. But since these lengths are so much shorter in the vacuum vibrator current, an initial frequency one-fifth as great, without the use of additional coils and condensers, produces far better results. But that an ordinarily constructed vibrator can attain a speed of 6,000 per minute, may be questioned, when it is remembered that induction coil vibrators work at a rate of but a little over 1,000. The difference lies in the fact, that the vacuum vibrator has no air pressure to impede its movements, and also that a much shorter space of time is required for a single complete interruption, because the actual mechanical movement can be much less, yet cause a complete break, and another cycle has begun. The speed of the vibrators is ascertained in two ways:—first, by comparing the musical note it produces with that of a pitch pipe, and secondly, by a visual arrangement constructed and operated as follows:

A shaft, supporting a wheel with one spoke, is rotated rapidly by hand, a series of multiplying gears being used, so that when the hand makes one revolution per second, the spoke makes twenty. When this apparatus is operated in a room, lighted only by a single vacuum tube, the spoke will appear stationary in one, two, or three positions, according to the rate of the vibrator. For example, if the spoke appears stationary in two positions, it indicates that it is illuminated twice in a single revolution, each image being due to a vibration.

Another subject, which has been a serious obstacle in former proposed systems, is that of impedance. The fact that the best conductors would cease to transmit current, seemed a difficulty almost insurmountable; yet it appears to be almost absent under these new conditions. For example, when a large coil is inserted in the high potential lead from the armature terminal to the tube, the effect on the light is surprisingly small. The result is the same, whether the wire be in the form of a magnet, or in a long exposed line. But, nevertheless, on account of line losses, it is advantageous to prevent condenser action by using a wire as small as possible, yet able to stand the strain it is subjected to when its insulation is being placed upon it. Although the light produced by a single wire is quite good, it is decidedly advisable for best results to use a return wire, because the fundamental frequency is so low.

The third subject—economy—is so large, and of such importance that I deem it expedient to make it the subject of a future paper when accurate measurements have been made. Indeed, it is second only—but it is second—to the nucleus of the whole investigation, *viz.*, getting light. The efficiency of the lighting tube is well established, due principally to the great amount of light accompanied by so little heat that it has by some been called "cold" light. The temperature of the gas within the tube varies with the density of the discharge from 12° to 132° C.; but even this is improved by the shorter wave lengths. These figures are extremely low, as compared with temperatures as high as 3,500° C., which must be reached by some substances in order that the light be white and the spectrum complete.

Owing to the peculiar characteristics of the current, the line losses are materially decreased, and the current flowing through the primary circuit is less when the tube is giving light, than when it is disconnected. There are almost no losses for motive power to disrupt the current, for the magnet is its own motor. But the greatest loss has always been in the disruptive discharge, —the spark. It is remarkable how easily several horse-power can be dissipated in the air through the intervention of a disruptive discharge. In this connection it should be borne in mind that magnetic blow-outs and air currents are merely heat dissipators, and increase the loss, while these losses are entirely obviated by the vacuum vibrator. The current can perform a

large amount of work on the air, unlimited in volume, with no apparent results; but when this volume is reduced to the compass of a small vibrator tube which remains perfectly cool in operation, does not the question of efficiency assume a different aspect?

Upon these reasons may be based logical conclusions, pointing to an enormous increase in efficiency over that of all other methods of obtaining light in tubes.

The theory which has just been considered, of course, is not limited to a simple vibrator, but applies broadly to any method of interrupting the flow of current through a high vacuum. This can be accomplished in a great variety of ways, although as far as simplicity of apparatus is concerned, the regular spring vibrator, in connection with a single magnet, probably cannot be improved upon.

The first deviation, however, is to use a very small magnet to vibrate the armature, and to connect this in series with a larger one to furnish the induction. But in these cases the power of the operating magnet depends on the current passing over the contact points; hence to make the light which is dependent on the contact points perfectly positive, the power should not be dependent upon them, and separate circuits suggest themselves—that is, cause an intermittent current to flow through the power magnet that does not flow through the contact points of the vibrator, which have in circuit with them the inductive magnet. Or the electrodes can be separated by mechanical jarring instead of magnetic power acting through the glass.

If an ordinarily constructed vibrator be attached to any form of rapidly oscillating mechanism, the contacts within will be opened and closed rapidly. But in order that the light be steady, the movements of the vibrator armature must be in step with the movements of its mechanical support. This is best accomplished by having the centre of the oscillation of the vibrating armature coincident with that of its oscillating support.

It is plain that it is unnecessary that the current be interrupted by a reciprocal motion only; a rotary motion is also applicable.

Fig. 31 shows a form somewhat analogous in operation to the simplest vibrator, except that the rotary momentum of the armature takes the place of a spring, and the break-wheel which furnishes the light, also acts as a commutator to the simple form of motor.

Another way of obtaining a rotary motion within a vacuum is to attach a pendant weight to a ratchet wheel, free to rotate upon a shaft attached to which is the brush. Since the shaft is rigidly sealed into the glass, it is evident that when the bulb be rotated by a motor, the brush will revolve around the break wheel. The fault with the device is that the pendulum will have a vibration of its own, causing the light to waver.

In order that the make-and-break devices dependent upon a rotary motion be absolutely positive, a rotating magnetic field has been utilized. Since the experimental side of these investigations has extended over but a few months, it is at present difficult to say upon which of these methods, developed, the system will take final form.

Returning once more to the subject of the light in the tube. It is interesting to note, that although the most intense light is

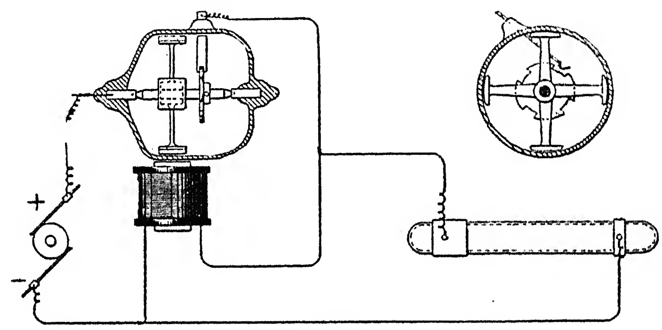


FIG. 31.

produced when both of the ends of the tube are connected to the electrodes, it is well known that light also results when the tube is merely placed near an electrode without actual contact. This phenomenon is known as lighting by induction.

If an inductive circuit, sufficiently powerful to brilliantly illuminate a tube four feet long, be transferred to one two feet long, there will appear at the center of this small tube a very intense thread of silvery white light, which undulates as if it were a material substance. It is interesting to contrast the color of the light of these tubes with that of an incandescent lamp. The reddish and wasteful glare of the latter, indicating heat waves, and the pure daylight white of the former, is immediately apparent. It is this difference in color that makes an efficiency calculation so difficult. It is very easy to ask the question :

How many watts per candle-power? But in most instances it indicates a lack of information on the part of the questioner. The question should be: How many watts per amount of light equivalent to one candle-power? But even this is not perfectly correct, because it is well nigh impossible to compare accurately lights of a different color and power of diffusion. When the *use* the light is to be put to is stated, the problem is much more simple. For instance, if it is to be used to read by, the range of legibility can be made the basis of comparison between the true glow lamp and the candle. This most popular form of illumination, from the 12th to the beginning of this century, is still the standard of illumination, although it is probable it will soon be deprived of this honor. I may be pardoned for calling your attention to the remarkable intensity of the light in these tubes, in connection with the statements repeatedly made by eminent scientists, that such intensity was an impossibility, and that efforts in this direction were comparable to those wasted on perpetual motion. It is merely another instance of history repeating itself, in that in all times the inertia of the learned has interfered more with the progress of science than has ignorance. Be it remembered that the commercial incandescent lamp was an acknowledged impossibility among scientific men, and that by them the proposed Atlantic cable was considered foolishness. If there be but one lesson taught by our times, it is, condemn nothing *new* in religion, science or art without thorough investigation, and even then be careful, because many suggestions, though of little value themselves, have led to great advancement. It is also well to remember that there is almost a creative force in the spirit that is earnest and courageous. The light having reached the intensity, as you see it in this tube, it is questionable whether much greater intensity is wanted. The vibrator, as applied to the electric bell, was the first practical application of electric power, and to-day we see the same vibrator in a "new light."

The very nature of the light, if it is to be counterpart of the ideal-daylight, is such that when a square inch of the surface of the tube emits as much light as that thrown into a room through an aperture one inch square, the want is satisfied. Then the desired illumination can be reached by multiplying the area and length of the tubes, and distributing them in the most advangeous manner, that is, so that the light will fall from all directions. When a considerable area is to be lighted, the most efficient light is the

one that is most equally distributed. However, there will always be a demand for units of light. Even this can be satisfied by using a tube of small calibre. This lamp is made by winding a small tube in the form of a spiral, its ends, to which the wires

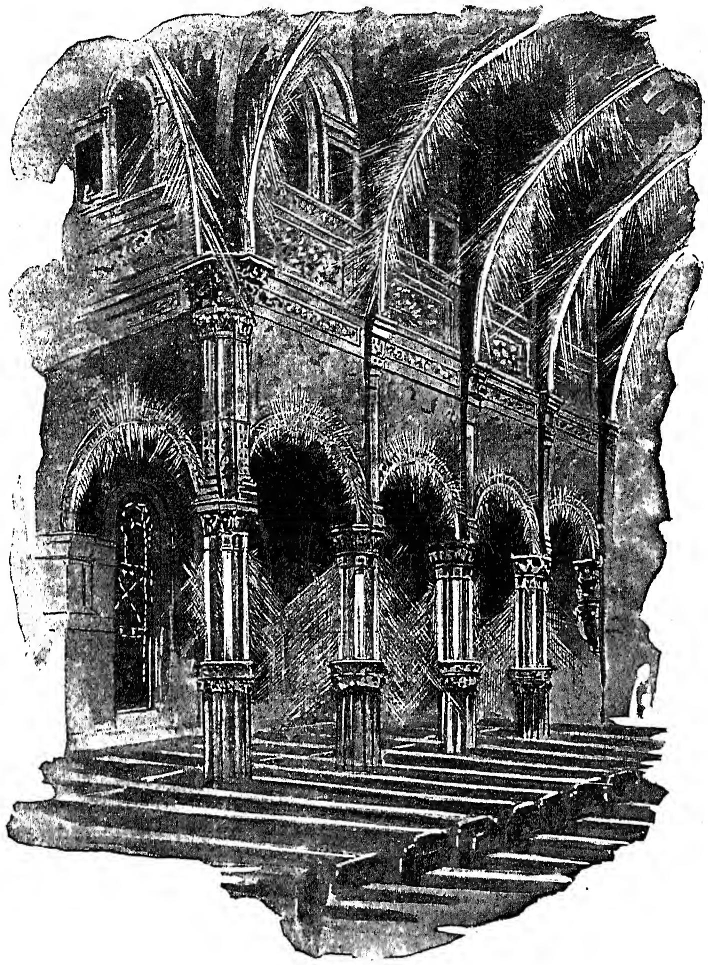


FIG. 32.

are attached, terminating in oblong bulbs three or four times the diameter of the small tube.

I have previously stated that the alphabet has been constructed of tubes of light. Here are the initials of the body I have the honor to address, A. I. E. E., in letters twelve inches high. The

delicate shades of these letters cannot fail to elicit admiration from all who love the beautiful.

The principle of breaking a circuit in a vacuum has many applications to a variety of uses. Among them may be mentioned, advertising signs, decorative electric lighting, electrotherapy, philosophical apparatus, theatrical effects, in the manufacture of ozone, in the kinetoscope, etc., etc.

But the greatest field will ultimately be that of general illumination. You have noticed the tubes extending around this hall. Undoubtedly this is the first time that lighting by tubes has been attempted on so large a scale. You will note the almost entire absence of shadows.

Fig. 32 illustrates what we are coming to in the way of church lighting. For some time past everything has pointed to the general adoption sooner or later of some such form of illumination, and since volumes of light can now be produced, and of commercial intensity, does it not indicate that already this light is a matter for serious practical consideration, and no longer a pyrotechnic curiosity?

But the only way in which one can form a comprehensive or appreciative idea of what advance in this line of work really means, is to compare the situation of to-day with that, not of a hundred years ago only, but with that of only twelve months ago, and note the contrast.

Discussion.

[Owing to lack of time at the meeting of April 22d, there was no opportunity to discuss Mr. Moore's paper. The following discussion took place at the General Meeting May 20th, 1896.]

Mr. C. P. Steinmetz:—Mr. President and Gentlemen. While reading, and afterwards listening to the presentation of the paper on "Recent Developments of Vacuum Tube Lighting," a number of points occurred to me with which I cannot agree. The foremost criticism which I have to make against the paper as a whole, is the same as that which I had to make only recently against another paper, namely, that it contains a number of vague claims and statements, without offering any proof for them.

Coming now to an analysis of the paper, I find on the second page a statement with which I must beg to disagree, namely, that the paper "represents radical departures in principle, in apparatus, and in the nature of the current."

The electrostatic discharge is produced by a sudden change in the electric circuit, consisting of a make and break of the circuit.

This is precisely the same method used since the early days of the Ruhmkorff coil, with the only exception that the interrupter is placed in a vacuum. The absence of a secondary coil is not new, but not desirable where the primary E. M. F. is very low, as a battery cell, as stated on page 95.

The allusion to the ponderous alternator, oil transformers, magnetic blasts, the millions of volts required, etc., is obviously gross exaggeration.

One disadvantage of this vacuum interrupter, compared with the condenser discharge across a spark-gap, has been already noticed by the lecturer. While the condenser discharge through the spark-gap makes its own frequency, with the vacuum interrupter the circuit has to be carefully adjusted to some condition of resonance. That the nature of the current produced by the vacuum interrupter does not differ in any way from that of any electrostatic discharge,—either oscillatory or steady, according to the circuit conditions,—is self-evident. The phenomena produced by this electrostatic discharge in vacuum tubes, and described in the paper, as the luminescence of the conductor and of the electrostatic field of force, the diffused glow at a different degree of vacuum, etc., have been observed many times before, and exhibited with nearly the same brilliancy, for instance, at the World's Fair in Chicago by Mr. Tesla. The use of external electrodes in vacuum tubes is old.

The luminescence of the interior of the spiral in no way contradicts Faraday's experiment, since the interior of an open spiral is obviously not the interior of a charged body, and Faraday's experiment, as is well known, refers only to electrostatic charges at rest, but not in motion.

What cause the lecturer has to claim an irregular period for the oscillations produced by the condenser discharge through an inductive circuit, I cannot see, since it was with such discharges that interference and nodal points of electro-magnetic waves have been observed. The wave length of an electric oscillation is not shorter if the wave is quicker, as stated by the lecturer. It is well known that the period of oscillation is entirely independent of the rapidity of the break, and only the amplitude can be affected thereby. Thus the conclusions drawn herefrom are erroneous.

The calculations on page 98, on the counter E. M. F. of the ether gap and its particular and radically novel features, are too fantastic to pass any scientific scrutiny.

A number of other statements in the paper are unintelligible, as, "natural period of a coil," or "governing the period of oscillation"—by the way, through the high resistance of the vacuum tube, the discharge is probably not oscillatory at all, but steady—or "compensating for the long wave length by a discharge coil," or the action of return wire, etc. Other remarks border on mysticism as, "striking nature's key-board to the tune of 500 trillions of waves per second," etc. In general it is to be

regretted that a large part of the paper is written in a style more befitting a sensational newspaper than a scientific or even technical society.

After criticising what the paper says, I may add a few words regarding what it does not give, but what such a paper, to be suited to such a body as the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, should give.

At the risk of being included by the lecturer amongst those lacking in information, I must nevertheless raise the all important question, "How many watts per candle power do you require"?

I cannot see any difficulty in determining this. Compare the light given in one metre distance by one of these tubes, with that of a standard candle, in the Bunsen photometer. The total candle power of the tube, to that of, say, an incandescent lamp, will be proportional to the area of the surface at one metre distance from the tube, to the area of a sphere of such a distance from the standard candle or lamp, as to give the same illumination of say 1 $cm.^2$ of a white surface. Or if you object to the difference of color, compare by means of colored screens the intensities, say at the points A, D and F of the spectrum.

Or taking a more concrete case: The lecture room was illuminated by 27 Geissler tubes of about 7 feet length and 2 inches diameter, instead of 22 incandescent lamps as usually. Compare the intensity of the light reflected by 1 $cm.^2$, white surface in either case, with a standard candle in the Bunsen photometer. This will give you an exact measure of equivalence. I should judge the Geissler tube light to be about equivalent to five or six 16 candle-power lamps. Now determine the amount of power taken from the supply dynamo. I saw a 7½ H. P. Crocker-Wheeler motor driving the dynamo, and from the hum of the motor it seemed to slow down noticeably if the load was thrown on the dynamo, and a 7½ H. P. Crocker-Wheeler 500-volt machine flashed badly, showing either a defect in the machine or an overload. This does not look like high efficiency. However, a public demonstration is not very suitable for such tests, but they should have been made before in the laboratory, and their results communicated to us here. Thus, while the paper is very interesting as showing improvements in Geissler tubes, or illumination by Geissler tubes, no data whatever are brought forward regarding the only and all important question: "How does the power required for lighting with Geissler tubes compare with that required for the same illumination by incandescent lamps?" I may add in closing that the paper would have been essentially improved if all reference to Galileo about the interference of the inertia of the learned with the progress of science, etc., had been omitted.

THE PRESIDENT:—There is a communication on this subject by Mr. J. P. Wintringham, which will be read.

MR. J. P. WINTRINGHAM:—This paper seems to have some interesting new matter, but the author has written a little carelessly, and I would note two or three lapses. He speaks of the counter E. M. F. of the air-gap. The effect is not a counter, but a direct E. M. F., and is due to self-induction, not to the air-gap.

He speaks of a high frequency oscillating discharge, which term has a special technical meaning among electricians, referring to the discharge of a leyden jar, etc.

He seems to deal with a branched circuit, the current of one branch being periodically interrupted, the current in the other branch being also periodic. There does not appear to be any charge and discharge or any oscillation involved.

What I have said applies to the lamps with two electrodes. Some of the lamps had only one, and the tubes were without any electrodes. Here there would be condenser action, charge and discharge. The question if it is oscillating would be determined by a formula put as follows by a very high authority:

The resistance must be less than $2\ A\sqrt{\frac{L}{C}}$ for the discharge to be oscillatory. $A =$ the velocity of light, 30 earth quadrants per second, or, say, in practical units, 30 ohms. As L, C and the resistance are not given, it can only be conjectured that the discharge may be oscillatory.

The time of an oscillation would be given by the formula

$$T = \frac{\pi}{A} \sqrt{L\ C}.$$

As A has a value 3×10^{10}, any ordinary value of $\pi\ \sqrt{L\ C}$ would leave T so small, and the number of oscillations per second so large that the number of makes and breaks of the contact maker would be of a different order of magnitude and entirely incommensurable with any high frequency effect.

So the expression, "when the rate of the vibrator is the same as the natural period of the coil or any of its harmonics" would seem to be meaningless.

DR. E. L. NICHOLS:—I was not fortunate enough to read this paper. As regards the probable efficiency of vacuum tube lighting, I may say however that we are not absolutely ignorant with regard to the efficiency of the Geissler tube discharge. Measurements were made in Zurich by Dr. Staub on the tube itself, in which he got the radiant efficiency, which ran very high, sometimes as high as 33 per cent. That means that the spectrum of this source is very largely luminous in its character. A few years later Prof. Angström made other measurements with the Geissler tube, in which he included the losses in the generator circuit. The heat lost in a generator circuit of such tube being from the very nature of the case large; where you use an interrupted circuit you may expect that. He found as a result of bolometric measurements that while the radiant efficiency some-

times reached 90 per cent., that only about eight per cent. of the energy expended in running such a tube was converted into light. This would seem to indicate that vacuum tube radiation, if produced by the means at present known to us, or, at least, by the means used by Prof. Angström, is not likely to be found, of extremely high efficiency. In order to secure a vacuum tube discharge which shall be of very much higher efficiency than other electric lights, you have got to devise some means of generating that discharge which shall be free from the losses which exist with any ordinary make and break apparatus.

MR. STEINMETZ:—About a year and a half ago I experimented somewhat in the same direction, and tried to measure the power consumed by Geissler tubes. While these tests have never been completed, due to more urgent work, I noticed that the efficiency of the Geissler tubes apparently depends to a large extent upon the frequency. With very high frequency, as by oscillating currents, the vacuum tube gave quite a noticeable amount of light while remaining perfectly cool. With a frequency of ordinary machine currents, however, when giving about the same amount of light, the tubes became noticeably warm.

MR. L. M. PINOLET:—In the absence of Mr. Moore, who is detained by sickness, I wish to explain that the slowing down of the motor, which has been referred to, was not caused by an overload but was due to the fact that the motor was run with the resistance of the starting box only partly cut out. Measurements made the night following the lecture, showed that the magnets with their vibrators took about one eighth of an ampere each, at a pressure of from 450 to 500 volts. Since then a change has been made in the apparatus which has reduced the current consumption to about one tenth of an ampere. In making these measurements, it was noticed that the brightest tubes took the least current.

PROF. ANTHONY:—I would like to know exactly where the ammeter was placed when these measurements were made?

MR. PINOLET:—It was placed in series with the vibrator and its magnet; whose current was to be measured.

MR. N. W. PERRY:—The statement that he was present when the test was made showing a consumption of energy in those 27 tubes being exceedingly small is interesting. I saw a gentleman present here a little while ago who I think could tell us just what the results were that were obtained and how the tests were made.

MR. WOLCOTT:—Mr. Moore told me last night that he took about forty watts per tube.

THE PRESIDENT:—Do you know how many tubes there were at the INSTITUTE meeting.

MR. PINOLET:—Twenty-seven.

MR. WOLCOTT:—In justice to Mr. Moore I will say that he has since succeeded in getting a much better quality of light than that exhibited before the INSTITUTE.

MR. CARL HERING:—In connection with this light, the question is often asked, how many candles are generated per watt. It seems to me that it is not quite right to put the question in that form, if it is intended to make a comparison with other forms of electric lighting. I agree with Mr. Moore in his statement that it is not altogether a question of watts per candle; the real question is, how much energy does it take to light up a given room with this and with the other systems, so that the effect is the same to the human eye. Except for photography, we generate light so as to be able to see, and therefore if the effect on the eye is the same, it does not make any difference to us how many candles of light there are generated in that room. It is well known that if a certain amount of flux of light is all concentrated in one small point, it becomes glaring and dazzling, and the retina of the eye contracts, thereby limiting the amount of light admitted into the eye, while if that same amount of light were distributed equally, as for instance when reflected from white walls or emitted from the surface of large Geissler tubes, it would have a much better effect on the eye, and would produce what would be called a much better illumination. I therefore do not think too much stress ought to be put on merely the number of candles produced per watt in this system, without also considering the very important factor of the distribution.

MR. A. E. KENNELLY:—Mr. President: It has given me great pleasure to witness the distinct advance that Mr. Moore has made over the Geissler tube, in the production of light by high tension discharges. This is a direction in which advance may be looked for, and in which we may hope that success will ultimately be achieved. Yet I think we should face the facts as they are, if only for the sake of having an accurate criterion as to how far progress is made, and I think we should face the question as to how much light is produced for a given quantity of power. Of the forty watts which are said to be supplied to one of Mr. Moore's tubes at the present time, we cannot believe that all are expended in light. It would seem probable that most of it would be expended in exciting the magnet whose discharge produces the light, in the same way that a fifty-candle power lamp absorbs fifty watts to produce a relatively small activity in visible light. The fact that the tube occupies an extended space and offers an extended surface, does not really come into the question. What we want to know is, how may units of light are produced from this tube, considered as a source of light, for the alleged forty watts which are supplied to it. That is a perfectly definite question capable of a perfectly definite answer, and if that answer can be given to us at the present time it will at least afford us a valuable criterion of the advance which Mr. Moore has made in the art of Geissler tube lighting.

MR. E. E. RIES:—I coincide perfectly with what Mr. Hering and Mr. Kennelly have said in discussing this new method of Mr.

Moore's, that it is not proper to compare it with existing standards of candle-power; that is to say, with systems of concentrated lighting to which we have been heretofore accustomed. The new system operates by diffused light; that is, a lesser intensity of light, given forth by a larger emitting area. Now, if it can be shown that the luminosity given by this method to an apartment or room is sufficient for practical purposes, that is, for the purposes of reading or working by, at a fairly economical cost, it will denote that a considerable advance has been made. I do not think it wise for the members of the INSTITUTE to condemn a new invention at the outset, simply because it has not arrived at that stage of efficiency which, if the theoretical principles underlying it are true and correct, it will probably eventually attain. All methods of artificial lighting are more or less inefficient. We have to spend a great deal of electrical energy in producing waste, either in the form of heat or chemical action, or both, in order to get some light. In the incandescent electric lamp a large waste of energy occurs, and in other sources of illumination similar wastes takes place. It is possible that in the vacuum tube system the waste incidental to the excitation of the magnet, and the other losses due to the resistance of the air-gap, etc., which appear to be the seat of the principal losses in this case, may eventually be minimized. I think it well to defer giving a hasty decision until that point can be determined.

If I recollect rightly, some years ago I saw a statement to the effect that Mr. Moore had been experimenting on a make-and-break in a vacuum containing a vibrator very similar to this, for the purpose of producing a regulating socket or controlling device for incandescent lighting by intermitting or interrupting the flow of current passing through the lamp filament more or less frequently. The method itself was not new, but the means of accomplishing it—that is, the making and breaking of the circuit in vacuum—was new. I am very glad to know that from that initial starting point this later achievement in tube lighting has been evolved. It is another one of those curious instances where an inventor originally starting out on one line of investigation discovers, turns up and develops something in an entirely different direction. I hope that future developments in this respect will show that something valuable may be expected from this new method of lighting.

MR. STEINMETZ:—I must protest, Mr. President, against the imputation that anyone wants to condemn this paper. What we want is to get exact data and not mere general statements. What we condemned was the entire absence of numerical data in the paper.

MR. CARL HERING:—We know the arc light is much more efficient in candles per watt than the incandescent light, yet we use the incandescent light, because we can distribute it. It is possible that even if in the light under discussion the amount of

energy required per candle power is greater than in the incandescent light, it might nevertheless be a more economical way of illuminating a room.

MR. PERRY :—That brings up another point. I think all of us who saw that exhibition were guessing as to how many incandescent lamps distributed judiciously through the hall would enable a person to read with equal ease. All the guesses that I heard gave a very small number. Mr. Hering's point was that the larger surface ought to give us better results. That is so, and measuring by incandescent lamps would not be unfair to the lamps and favorable for the tube. The only data we have as to the power consumed in those tubes was the behavior of the generating apparatus. There are a great many who saw those instruments work. I could see the motor part of the time, and the rest of the time I could hear it, and my impression was that there was not less than ten horse power generated. If we hear a $7\frac{1}{2}$ H. P. motor laboring as that one was, it is a pretty sure indication that energy is being generated, and this must have gone somewhere. If we divide it up among those 27 tubes and it gives no more diffused light than a comparatively few 16 candle power incandescent lamps then it would seem to indicate that the light was a very expensive light.

MR. KENNELLY :—While I agree in some respects with the last speaker, I think that matter ought to be viewed from a somewhat different standpoint. We have here a new means of electric lighting. I do not suppose that anybody thinks the method has advanced to a practical stage. That is not the question at present. The question is, what is the possibility of this means becoming better in the future? In that point of view it becomes of interest to know just how the facts stand. These tubes produce light, and the light is capable of being measured. They absorb power that is capable of being measured. At the INSTITUTE the other night, by an arrangement of white screens behind the tubes and gauze above them, an illuminating effect was produced in the hall. But we want to know how much light these tubes produce for a given quantity of energy. We shall then have something by which to judge of future progress.

PROF. WILLIAM A. ANTHONY :—The candle power is simply a measure of the flux of light, whether it comes from a small point or a large one. If we hang up the arc light, what we observe is the illumination upon a little screen within the photometer carriage, and we make that illumination equal to that of the standard. There is no change in the size of the retina here. The brilliancy of the arc lamp does not directly affect the eye. But we compare its illumination on a screen, with the illumination of another source of light and so measure the intensity. The fact that we cannot use the arc lamp itself with the same efficiency in illuminating a given space that we can a smaller source of light, has no bearing upon this

point that I see. In the case of illumination by means of these tubes, the tubes present a large surface. It may present advantages as regards the distribution, but that makes no difference with the amount of candle power per watt. In regard to the results the other night at the INSTITUTE meeting, I took pains while the tubes were being used to observe how well I could see to read, and tried to compare that as well as I could with the ease with which I could read by means of the incandescent lamp. When the 27 tubes were in use it was certainly very much more difficult to read than when the usual lamps were employed for lighting the hall. I think that was evident to everybody. How much more difficult it was, I could not say. When the one large tube in the front of the hall was doing its very best, the one large tube with a reflector behind it, I could see to read nearly as well as when the reading lamp that was used on the desk was turned around, as it sometimes was, towards the hall. I was not able to see as well by the one large tube as I was by that single reading lamp. That, it seems to me, is a fair comparison of the illuminating power of those two sources, and it is exactly what we want to know about. The absence of such information is what we are criticising in Mr, Moore's paper. Mr. Moore has certainly done a great deal, and has made a great advance in vacuum tube lighting. He has accomplished something that we have never seen done before. But at the same time we would like to know just how much light he will give us per unit of energy expended.

DR. NICHOLS:—If the photometer is not for the purpose of measuring the illuminating power of light what is it good for? The work of the committee on light standards would seem to be wasted if when we got through we are not going to be able to measure the power of all sorts of light. The light which falls upon the disk of the photometer produces there an effect which is entirely independent of the area of the source. It does not make any difference to the man who is looking at the photometer whether the source is concentrated or not. If you test your light by some other criterion, the question of diffusion may come in. If you are standing with your back to an arc light you cannot read in the shadow. There is a certain benefit which is obtained by distributing your light so as to avoid showy shadows. All other benefits than that of getting rid of black shadows is entirely fictitious, unless the bad effect upon the retina of gazing at intense sources of light be considered.

MR. RIES:—It does not seem to me to be a question of relative candle power so much as the amount of energy required to produce the required light, or, more properly speaking, the utility of the light in comparison with its cost. When it comes to a question of measuring light by candle power that is one thing, but so far as its *commercial* use is concerned that is quite a different thing. If we take an arc light, which will give us 2,000 candle power

with an expenditure of one horse power of mechanical energy, we will, by expending the same amount of energy, be able to get at the most, about 200 candle power out of a number of incandescent lamps, yet because the latter are only one-tenth as efficient as the arc lamp, that is no reason why we should dispense with incandescent lamps, nor do we dispense with them upon that account. In the case just cited, we sacrifice a good deal in efficiency for the sake of subdivision and better distribution of the light, and it seems to me the vacuum tube method holds forth the promise of a still further refinement in this direction and in all probability at a far less sacrifice of economy. If vacuum tube lighting can be brought in the future into commercial use for the lighting of the interior of rooms it will have a field peculiarly its own. It will not drive out the electric arc light or the incandescent light, but it will open up a new field for ornamental and decorative lighting effects where intense luminosity is not required, and it is perfectly conceivable that with a satisfactory means of vacuum tube lighting, which of course is not concentrated like that of the arc and incandescent light but is capable of a more uniform, and for many purposes a more desirable diffusion, a great many practical uses may be found for it. From that point of view I say that it is not so much a question at the outset to criticise Mr. Moore's new method, or his improvement in the Geissler tube method, simply because at present, from our lack of information, we have not the data which shows us exactly what it will cost. If the cost is reasonable—even if it should be greater than the cost of incandescent lighting—it may still have a future before it. It seems to me that the question now is, to discuss the matter from the standpoint of an improvement in lighting rather than to criticise it at the outset too strongly from the point of view of its economical advantage over other forms of light.

MR. WOLCOTT:—One reason why the members have all taken the stand of the efficiency of this light is that the paper spoke of the incandescent light as very wasteful. The idea was that this was a cold light which took very little energy.

MR. KENNELLY:—I do not desire to criticise Mr. Moore's work in this direction, but we want to know just how much to admire it. If we have from Mr. Moore a statement of how many candle power he can produce, why, we obtain a criterion by which we can measure his success.

MR. HOWELL:—I do not think Mr. Moore would have the slightest objection to telling us all he knows about this. I do not think he would have any objection to answering every question that has been asked here today about it. There is one thing that was brought out by Mr. Moore that night which has not been touched upon, and that is the difference in color between the vacuum tube light and the incandescent light. You will remember that he held up a tube in a reflector, and alongside of it his

reading light, to illustrate the difference in color, stating it was his belief that the color of the tube light was very much superior to that of the incandescent light. It was my impression that the color was a very poor one, and that the whiteness, or greenness as I called it, of the light, was a very great disadvantage to it. I think the disadvantage of that whitish light has been shown by Dr. Birchmore in a series of articles in *The Electrical Engineer* in which he shows the action of different colored lights upon the retina of the eye, showing that the rays from the violet end of the spectrum rather contracted the pupil very much more than rays of light from the lower end of the spectrum. The contrast which he made between his light and the other was a strong argument in my mind against it.

THE PRESIDENT:—Mr. Moore has just come in. We should like to hear from him.

MR. D. MCFARLAN MOORE:—Mr. President and Gentlemen: Owing to a bad cold it will be difficult for me to make myself heard. The ideal light will undoubtedly only be reached when we can produce by artificial means a light which is the same as daylight. Since my method of vacuum tube lighting was exhibited at the last INSTITUTE meeting, improvements have been made in it so that I can produce at this moment tubes with a daylight spectrum. The vacuum tube method is the only one which presents itself for our use, in order to produce a light which will be anywhere near a counterpart of daylight,—a light which will be radiated from long tubes. In relation to efficiency, of course it was understood that the paper was presented merely to call attention to the advance that has been made. In order to appreciate that advance, a person must understand how very inefficient previous experiments and methods have been. The vacuum vibrator is undoubtedly a device which has greatly improved the economy of tube lighting over previous systems. Previous methods used a break in an electric circuit in the open air in order to produce the secondary current which would have certain properties necessary to produce the agitation in the tube and cause the light. In all those systems the break came into the system somewhere. Anybody who has experimented with a break in the open air, knows that it is very remarkable how much energy can be wasted. An enormous amount of power is dissipated in the open air break. Now, when that arc is placed in a vacuum which remains perfectly cool, it is apparent without any further discussion that there results an enormous saving. During the test at the INSTITUTE which has been referred to, the apparatus was run without the slightest difficulty and without anything getting out of order. A number of tests were made by the parties who were present, as to the range of legibility. That was the only way by which we could get any idea of the consumption of power necessary to produce a quantity of "cold" light, the equivalent of which could be gauged by incandescent

light. It was undoubtedly a fact that objects on the wall of the INSTITUTE could be discerned a great deal better with the tube lighting than they could with the 25 incandescent lamps. As to the power consumed, there was at no time more than two amperes used. Now, that is the only basis we have to calculate from, as far as power is concerned, viz., that you could distinguish objects better with two amperes, which is of course a less amount of power than required by 25 incandescent lamps. Since then I have well nigh cut the power required in half, and also increased the quality of the light. In fact, it is difficult to say just what power would be required to produce the same amount of light with the break in the open air; but if the same amount of light were to be produced with the break in the open air and by the inductive method alone, I am not exaggerating to say that the increase of efficiency is about eleven million per cent. This sounds outlandish, but it is a fact.

I do not know that there is anything more for me to say, except that I will take great pleasure in explaining my apparatus to anyone who wishes to examine it. I can add that the spectrum from my tubes is absolutely continuous, something never obtained before from vacuum tubes.

MR. WOLCOTT:—I would like to ask Mr. Moore one question. On page 86 of his paper he says: "As a device for transforming electrical energy into light, the vacuum tube is very efficient. The majority of authorities place it at about 70 per cent. and the incandescent lamp at two per cent." I would like to know what is the basis for that. Does he mean 70 per cent. of the energy which is supplied to the electrodes appear as luminous radiation?

MR. MOORE:—Yes, undoubtedly. I wish also to add that the idea in showing the difference between the incandescent light and the vacuum tube light was merely to show that the vacuum tube light had very few red and wasteful heat waves. With reference to efficiency, of course it is understood that I referred to the amount of power consumed at the terminals of the lamp, and that this system of lighting, without heat, is merely a step in advance. We must come to it, and rapidly, and it is merely a step in that direction. The heat is not all removed but we are approaching that end.

MR. WOLCOTT:—I have never heard that a Geissler tube had 70 per cent. efficiency.

MR. MOORE:—I think Dr. Nichols can give you some points on that subject.

DR. NICHOLS:—The figures referred to are doubtless those to which I referred a few moments ago. Dr. Staub had a Geissler tube in an ice calorimeter, so that he was enabled to allow the luminous rays to escape. Then he blackened the walls of the tube and absorbed it all, and he found in that way about thirty-three per cent. efficiency. On the other hand, in the second set of

observations—those of Prof. Angström—comparisons were made between the luminous radiation, produced in a Geissler tube, and the total amount of energy electrically measured necessary to generate that light in the tube, the efficiency in such tubes thus tested was about eight per cent. This second quantity, which would seem to limit the total efficiency of the Geissler discharge at less than ten per cent. is a question of course of the method of generation, and if you can reduce the loss in the outside circuit then the luminous efficiency may be greatly increased.

MR. WOLCOTT:—Even at ten per cent?

DR. NICHOLS:—You have to divide that by the fraction of light-giving energy in it.

PROF. ANTHONY:—It seems to me that a good place to put a voltmeter and an ammeter is in the circuit that drives the motor. If we put the voltmeter and ammeter in the circuit which drives the motor, then all question as to the measurement, by means of the ordinary instruments, of this extremely fluctuating current that goes into the tube, as to whether that gives us an accurate measurement or not, would be disposed of. We could tell then very nearly what amount of energy is required to operate the tube.

[At this point the President requested Vice-President Steinmetz to take the Chair.]

THE CHAIRMAN:—I observe that Mr. Wetzler is present and some of the gentlemen have expressed a desire to hear him on this subject. Will Mr. Wetzler favor us?

MR. JOSEPH WETZLER:—The tests made by myself on Mr. Moore's apparatus were carried out as follows: A Weston direct current milliammeter, A shown in Fig. 33, was placed in series with the inductance coil employed by Mr. Moore, and the potential was measured by a Weston alternating and direct current type voltmeter, V, connected directly across the leads. The results of one set of tests was as follows:–

	Volts.	Milli-amperes.	Watts.
Tube No. 1, good tube	490	110	53.9
" "	470	110	51.7
Tube No. 2, poor tube, faint light	480	160	76.8
" "	480	160	76.8
Tube No. 3, good tube	500	85	42.5
" "	485	110	53.3
" "	485	110	53.3
" "	485	110	53.3
" "	490	110	53.9
	490	110	53.9
Tube No. 4, good tube	490	108	52.9
" "	485	108	52.4
" "	470	108	50.7
" "	490	110	53.9
" "	470	105	49.3
Tube No. 5, good tube	500	*88	44.0
" "	500	*88	44.0

*With momentary rise to 120.

In other tests which I made I found some tubes that gave much lower watt readings than any of the above; thus one read as low as 30.5 watts. The above results may therefore be considered a very fair average. No special preparation, so far as I am aware, was made for these tests by Mr. Moore, and all were taken in his Exposition booth while the apparatus was being shown publicly.

MR. RIES:—I should like to ask Mr. Wetzler what was the length of that tube on which the 42.5 watts reading was taken?

MR. WETZLER:—All those tubes were about seven feet long. In fact they were the same tubes that were used at the INSTITUTE lecture. No measurements were made on the illuminating power of the tubes. They were all in good condition except the one which I have referred to as a bad tube, which was evidently lacking in brightness.

MR. RIES:—Then one of those seven-foot tubes took practically as much energy as an ordinary 16-candle power incandescent lamp?

MR. WETZLER:—Yes sir.

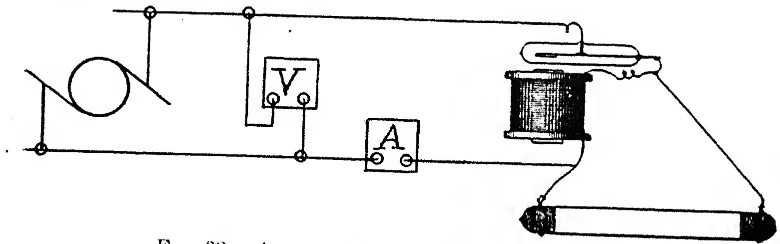


FIG. 33. ARRANGEMENT OF TESTING APPARATUS.

MR. MOORE:—I believe it was stated by somebody before I arrived, that the motor appeared to be overloaded. It is quite remarkable, when you consider all the motor experts there were at that meeting, that every one presumed that the motor was overloaded, and it only shows how unsafe it is to be too sure of anything. When the dynamo field rheostat arrived it was found when it was connected in circuit that you could turn its handle all the way on or off without affecting the voltage in the slightest degree. Therefore, it was a useless piece of apparatus, and it was not used. The motor starting box had seven points for varying its resistance. Now, at no time during the evening were more than three points cut in, in the motor starting box, and I think most of the time only two. Of course, anybody who is familiar with this kind of apparatus knows very well that when you have a motor with only two-sevenths of its starting box cut out when you put a slight load on it, it will lose its voltage. That was the whole truth of the matter. Of course if the attendant had turned the starting box all the way around and cut it all out then it would have been all right as it was the following night.

DR. S. S. WHEELER:—I attended the INSTITUTE meeting at which the new light was shown, and I thought I noticed a

squeaking, indicating that the belt between the dynamo and the motor was slipping. I would like to ask Mr. Moore about that.

MR. MOORE:—Dr. Wheeler must have been mistaken. A number of men who are competent to know how tight a belt should be stretched, all agreed that the belt was all right. It worked perfectly, merely by throwing the resistance of the starting box out.

PROF. ANTHONY:—During one stage of the experiments I knew the motor starting box was not all cut out. That was when the first attempt was made to start up the tubes for lighting the hall. Later I saw the assistant go and turn it out, and I supposed it was fully turned out; I feel quite sure that it was. In regard to the laboring of the machine, it was not the laboring of a machine that was working with the starting box partly turned on, but it was the laboring of a machine that was overloaded.

MR. MOORE:—In response to that I must say that Prof. Anthony is entirely mistaken. I have the very best evidence to show that that starting box was not entirely cut out. I know that the following evening the apparatus worked perfectly and on several other occasions. Indeed it is working perfectly now, or was a few days ago before the motor was changed.

MR. KENNELLY:—Fortunately it is not a matter of any consequence, gentlemen, whether the motor was overloaded or not. If these tubes take from 40 to 60 watts we know they will give their regular complement of light with that amount of power. Therefore, we can fortunately consign to oblivion the question whether the motor was overloaded or not.

PROF. ANTHONY:—I don't feel so sure of that.

MR. WETZLER:—I would like to state that the measurements were made with two different types of instruments and on different days. One was a direct reading ammeter, and the other was an alternating and direct reading type of Weston instrument.

PROF. ANTHONY:—I am not certain that in that interrupted circuit any of those instruments will properly measure the energy consumed. The sort of current that is obtained by that interruption is something totally different from any alternating current, and it seems to me that it may very well be that the voltmeter and the ammeter would not measure truly.

MR. PERRY:—I have felt the same way that Prof. Anthony has, that neither the direct nor the alternating current measuring instruments would be likely to give us results which would not be open to some question. There is another question that I would like to ask Mr. Wetzler. I think he stated somewhere that the total number of tubes consumed a given total amount of energy. I think I saw that in some editorial of his.

MR. WETZLER:—I do not recall the exact language.

MR. PERRY:—I understood these tubes were each on a separate circuit. Now, were the tests of all the tubes made on instruments simultaneously or under what conditions?

MR. WETZLER:—While all the tubes were operating I tested one of them at a time. Each tube being on a circuit of its own I would naturally measure one at a time.

MR. CARL HERING:—Perhaps a voltameter might be a much better instrument for such currents than an ammeter; if so, a simple Edison meter might answer the purpose.

MR. MOORE:—Mr. Wetzler has told you that he measured but one vibrator at a time. I measured a number all at the same time, and found the current consumption proportionately the same. With reference to Prof. Anthony, he seems to be determined that that motor was overloaded. I may be able to help him out in that matter. As is well known, when a motor under those conditions has a load placed upon it of course it will drop in its speed. Then the dynamo may drop from 450 down to less than 100 volts and of course the vibrators would stop vibrating, and the moment they do that, there is a short circuit through the coil and immediately the amperage would run up enormously. So that may well explain why the motor appeared to be overloaded. The fact of the matter is, however. that it never was overloaded. I wish to state once more that this matter of efficiency is absolutely secondary. I was able to light up a hall, the first hall in the world to be so lighted. I have placed my work before you claiming an advance in the art, irrespective of the question of efficiency, although I have nothing to be ashamed of in that direction, in fact the opposite.

[COMMUNICATED AFTER ADJOURNMENT BY PROF. ANTHONY.]

The doubt which I expressed in the discussion at the meeting in regard to the truth of the indications of the Weston instrument when used to measure interrupted circuits was, set at rest by experiments I was able to make on Wednesday evening after the meeting of the INSTITUTE, and of which I gave an account in a report to Mr. Moore which is printed in full below.

NEW YORK, May 27, 1896.

D. MCFARLAN MOORE,

DEAR SIR:—On Wednesday evening, May 20th, in company with Prof. E. L. Nichols and Mr. Nelson W. Perry, I made a test of the power absorbed by the vacuum tube light in your exhibit at the Electrical Exposition, with results as given below:

A Weston ammeter was placed in the circuit leading to the motor used for driving the generator which furnished the current for the vacuum tubes, and a Weston voltmeter was placed across the motor terminals. These instruments were read at frequent intervals, while one of us in the booth below noted the time of turning on and off the light. Comparing notes, we found that when the tubes were all off, the motor consumed 12.5 to 13 amperes at 110 volts as a constant load. When the tubes were all on, the motor consumed 22 amperes at 108.5 volts.

The motor therefore comsumed, when tubes were not running . . 1400 watts.
When tubes were all running . . . . 2380 "
Due to tubes . . . . 980 "

There were in operation 14 tubes, $7\frac{1}{2}$ feet long, $1\frac{3}{4}$ inches diameter, 1 tube somewhat shorter but $2\frac{1}{2}$ inches diameter, and a few tubes of special designs.

We estimated that the whole was an equivalent of 16 of the $1\frac{3}{4}$ inch tubes. This gives 61 watts per tube applied to the motor terminals. Assuming that 80 per cent. of this energy was delivered to the tubes from the generator terminals, the power consumed by each tube is 49 watts.

This is practically the result obtained by Mr. Wetzler by direct measurement of the energy consumed by the tubes, and disposes of the question raised by the writer as to the reliance to be placed upon the indications of a Weston ammeter in circuit with a vibrating interrupter.

The only question now is, as to the intensity of the light obtained. Of this it was impossible under the conditions to make any reliable estimate. Our results simply show that the light of the vacuum tubes, as exhibited in your booth at the exposition, is obtained at an expense of a little more than one horse power.

The instruments used in making these measurements were kindly loaned for the purpose by Mr. Pionnie in charge of the Weston Electrical Instrument Co.'s exhibit, who gave us his assistance in making the necessary connections and in taking readings.

W. A. ANTHONY.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

ANNUAL MEETING.

New York, May 19th, 1896.

The Annual Meeting of the INSTITUTE was held at the New York Industrial Building, corner Lexington Avenue and 43rd Street, May 19th, 1896, and was called to order by President Duncan at 4 P. M.

THE PRESIDENT.—The first business in order will be the appointment of two tellers to count the ballots. I will appoint as tellers Mr. Townsend Wolcott and Mr. Wm. J. Hammer.

The next thing in order is the report of the Council and the report of the Treasurer.

The following reports were then read by the Secretary:

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

REPORT OF COUNCIL FOR THE YEAR ENDING APRIL 30TH, 1896.

As required by the Constitution, the Council submits for the information of the membership a report of the work of the INSTITUTE during the past year.

The Council has held ten regular meetings and one special meeting, at which the average attendance has been ten. The General Meeting of the INSTITUTE was held at Niagara Falls, June 25th to 28th.

In compliance with a request, by petition, of twenty members and associate members, residing on the Pacific Coast, Prof. F. A, C. Perrine has been appointed Local Secretary for that region. In this capacity, Prof. Perrine has rendered efficient assistance in caring for applications for membership received from that district.

Upon his election as a manager, last year, Mr. B. J. Arnold deemed it

advisable to resign the office of Local Honorary Secretary at Chicago and Prof. W. M. Stine was appointed to fill the vacancy.

In compliance with a request from twenty members and associate members, a committee was appointed to submit a new design for a badge which would meet the approval of members and be more generally worn by them. A design has been prepared under the direction of this committee and will be submitted for consideration hereafter.

The amount of time consumed in the counting of the ballots at the annual election led to the appointment of a committee, of which Mr. James Hamblet was Chairman, to revise the rule governing this procedure. The report of this committee was received and referred to the Committee on Incorporations, as other amendments to the Rules were under consideration.

A Committee on Incorporation, of which Mr. W. B. Vansize is chairman, was appointed in December, to consider the incorporation of the INSTITUTE and any changes in the Rules that might be necessary. The whole matter was carefully investigated and the INSTITUTE was accordingly incorporated March 16th, 1896, under the laws of the state of New York. The new Constitution, amended as was necessary in order to conform with the State law, has been printed and distributed to the membership. This action was taken in accordance with the following resolution adopted at the meeting of Council January 22nd 1896:

"It is voted that the President be directed to call a special meeting of "the INSTITUTE to consider a proposition to incorporate the INSTITUTE under "the Membership Corporation Law of the State of New York. (Laws of "1895, Chap. 559, Section 5.)"

This meeting was called for February 26th, at which the Council was authorized by a unanimous vote to proceed with the work of incorporation.

The Council appointed Dr. Francis B. Crocker a delegate to represent the INSTITUTE in the National Conference on Standard Electrical Rules which was held in New York City, March 18th and 19th. This conference resulted in a permanent organization in which the Council subsequently voted to continue the representation of the INSTITUTE for one year, with Dr. Crocker as its delegate.

The National Electrical Exposition Company having extended an invitation to the INSTITUTE to hold its May meetings in the Industrial Building, the proposition was accepted by the Council, and the Secretary was instructed to secure the necessary facilities.

The total membership at the close of last year's report was 944, classified as follows:

Honorary Members	2	
Members	281	
Associate Members	661	
	944	
Associate Members elected May 1st, 1895, to April 30th, 1896	143	
	1087	
Restored to Membership	3	
Total		1090

The following resignations have been received during the year, and accepted as in good standing :

J. MURRAY MITCHELL,
F. S. CALDWELL,
R. L. SELDEN, JR.,
M. G. STRATTON,
H. H. EUSTIS,
C. E. STUMP,
E. H. ROGERS,
B. W. COLLEY,
J. A. BARRETT,
J. P. MAGENIS,
C. E. POTTS,
F. G. WATERHOUSE,
C. GESSEAUME,
G. W. MANSFIELD,
F. W. CUSHING,
H. HOLLERITH,
J. P. McKINSTRY.

Total resignations........................ 17

There have been the following deaths during the year :

FRANKLIN LEONARD POPE,
HENRY W. FRYE,
WILLIAM BOARDMAN TOBEY.
HOLBROOK CUSHMAN,
W. T. M. MOTTRAM,

Total deaths	5	
Dropped as delinquents	16	
Elections cancelled	3	
Elected but not qualified	14	
		55
		1035

Leaving a total membership of 1035 on April 30th, 1896, (a net gain of 91) classified as follows :

Honorary Members	2
Members	333
Associate Members	700
	1035

A list of the members elected during the year accompanies this report. The names are printed in the TRANSACTIONS.

The reports of the Secretary and of the Treasurer, show in detail the financial standing of the Institute at the close of the fiscal year, together with a detailed statement of receipts and expenses during the year.

SECRETARY'S BALANCE SHEET.

FOR THE FISCAL YEAR ENDING APRIL 30, 1896.

Dr.		*Cr.*	
To balance from 1894	$ 15 20	By cash to Treasurer	$11,262 90
Receipts for the year	11,267 90	Secretary's Balance on hand	20 20
	11,283 10		11,283 10

ITEMIZED STATEMENT OF RECEIPTS AND EXPENSES OF THE INSTITUTE.

For Fiscal Year Ending April 30, 1896.

General Account.

Receipts.

Treasurer's Balance from previous year	$186 28
Secretary's " " "	15 20
Typewriting	8 25
Entrance Fees	660 00
Life Membership (A. E. Kennelly, J. Bijur, R. R. Harvey, J. Stanford Brown)	400 00
Past Dues	694 16
Current Dues	7,903 13
Advance Dues	205 00
Electrotypes Sold	191 21
Transactions Sold	397 76
Transactions Subscribers	175 00
Advertising	144 45
Received for Binding Transactions	33 63
" " Badges	149 08
" " Certificates	58 10
" " Congress-Book	212 13
Reprints Vol. 4	36 00
Total,	$11,469 38

Expenses.

Repairs	$5 70
Incorporation	11 05
Extra Clerk	5 75
Chicago Meetings	111 00
Library	2 30
Directory	7 50
Ice	8 35
Duties	2 60
Engrossing Resolutions	26 00
Laundry	6 75
Office Expenses	25 53
Office Fixtures	42 60
Express	53 64
Telegrams	4 45
Stenography and Typewriting	987 75
Stationary and Miscellaneous Printing	912 56
Postage	679 33
Messenger Service	5 48
Salary Account	2,499 98
Meeting Expenses	390 39
Rent of Office and Auditorium	1,200 00
Engraving and Electrotyping	655 96
Publishing Transactions	2,972 01
Binding Transactions and Periodicals	280 93
Paid for Badges	165 00
Paid for Certificates	28 25
Congress Book	118 33
Secretary's Balance to next year	20 20
Treasurer's " " " "	239 99
Total,	$11,469 38

The outstanding current bills against the Institute, April 30, amounted to ... $769 13
Due the Institute and collectible probably ... 674 80

Property in New York City according to inventory, May 1, 1896.

Office furniture and fittings	$187 75
Transactions on hand	1,226 00
Congress Books	819 00
Library	200 00
	$2,432 75

Of the above, there has been purchased during the year, office fittings amounting to $25.50. The inventory has been made at a low valuation and does not include the Transactions of 1895.

The indebtedness of $2,511.29 brought over from last year, has been paid, as well as all bills of the current year up to April, being all that were in hand at the last meeting of the Finance Committee, excepting one of $30 held for cause, and another of $208.33 not due until May. The payment of these old bills necessarily carried over, makes an apparent increase in expenses over previous years.

Respectfully submitted for the Council,

RALPH W. POPE,
Secretary.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.
New York, May 19th, 1896.

TREASURER'S REPORT.

FROM MAY 1, 1895, TO MAY 1, 1896.

GEORGE A. HAMILTON, TREASURER, in account with
AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS

Dr.

Balance from May, 1895	$186 28	
Received from Secretary, May 1, 1895 to May 1, 1896	11,262 90	$11,449 18

Cr.

Payments from May 1, 1895 to May 1, 1896, on warrants from Secretary, Nos. 618 to 759, inclusive	$11,209 19	
Balance to new account	239 99	$11,449 18
Balance on hand, General Fund, May 1, 1896		239 99

BUILDING FUND.

Balance as per last report	$850 00	
Interest accrued to May 1, 1896, 3 per cent. to May 14, 1892 and 2 per cent thereafter	89 48	$939 48

Cash book and warrants herewith for audit. Vouchers are in the hands of the Secretary, to whom they are returned for filing after payment.

GEORGE A. HAMILTON,
Treasurer.

New York, May 19th, 1896.

It was voted that the reports be accepted and placed on file.

THE PRESIDENT.—Gentlemen, the counting of the ballots will take so long that I think it would be well to defer the report from the tellers until to-morrow morning. Therefore, I will declare this meeting adjourned until 10 o'clock to-morrow morning.

[Adjourned.]

ASSOCIATE MEMBERS ELECTED AND TRANSFERRED.

Council Meeting, May 19th, 1896.

Name.	Address.	Endorsed by.
HANCHETT, GEO. T.	Editor, with The W. J. Johnston Co., 253 Broadway, N. Y.; residence, Hackensack, N. J.	Townsend Wolcott. W. D. Weaver. Edward Caldwell.
HULL, S. P.	Chief Electrician of Hudson Div. N. Y. C. & H. R. R. Co., Poughkeepsie, N. Y.	T. D. Bunce. J. C. Chamberlain. A. E. Wiener.
LENZ, CHARLES	Draughtsman, Brooklyn Union Gas Co., 137 Nevins Street, Brooklyn, N. Y.	Jos. Wetzler. T. C. Martin. Max Osterberg.

Martin, James A.	Superintendent, The E. G. Bernard Co., 43 Fourth Street, Troy, N. Y.	E. G. Bernard. Edward Caldwell. W. D. Weaver.
Schwabe, Walter P.	Electrician, Rutherford, Boiling Springs and Carlstadt Electric Co., P. O. Box, 54 Carlstadt, N. J.	Walter M. Petty. Albert Buys. John E. Lloyd.

Total, 5.

APPLICATIONS FOR TRANSFER FROM ASSOCIATE TO FULL MEMBERSHIP. FINAL ACTION.

Approved by Board of Examiners, April 15th, 1896.

Roller, John E.	Lieut. U. S. Navy, in charge of Inspection and Installation, U. S. Navy Yard, New York.
Edgar, Chas. L.	General Manager and Chief Engineer, Edison Electric Illuminating Co., Boston, Mass.
Breithaupt, E. Carl	Electrical Engineer, Berlin, Ont.
Sever, George F.	Instructor in Electrical Engineering, Columbia College, New York City.
Harrington, W. E.	Electric Railway Engineer, Camden, N. J.; 1112 Sansom St., Philadelphia.
Bedell, Frederick	Assistant Professor in Physics, Cornell University, Ithaca, N. Y.

Total, 6.

GENERAL MEETING.

MORNING SESSION.

New York, May 20th, 1896.

The President.—The first business this morning is to listen to a report of the tellers.

The tellers reported, through Mr. Townsend Wolcott, as follows:

Report of Tellers

Annual Meeting at New York City, May 19th, 1896

FOR PRESIDENT.

Total Number of Votes Cast 330

Dr. Louis Duncan	287	Prof. Edward L. Nichols	2
Dr. F. B. Crocker	28	Charles F. Brush	1
A. E. Kennelly	4	F. J. Sprague	1
Thomas D. Lockwood	3	C. P. Steinmetz	1
Elihu Thomson	3		

Total...................... 330

FOR VICE-PRESIDENTS.

Total Votes Cast.................... 975

C. P. Steinmetz	307	Elihu Thomson	1
H. J. Ryan	306	T. A. Edison	1
W. M. Stine	267	E. R. Weeks	1
Elisha Gray	10	S. Sheldon	1
Dr. Louis Bell	8	A. L. Rohrer	1
Nikola Tesla	7	O. T. Crosby	1
C. R. Cross	6	E. W. Rice, Jr.	1
J. J. Carty	6	W. E. Geyer	1
E. L. Nichols	5	A. V. Abbott	1
A. E. Kennelly	4	Geo. Cutter	1
C. S. Bradley	3	L. B. Stillwell	1
W. J. Jenks	3	H. A. Rowland	1
R. H. Pierce	3	F. W. Darlington	1
W. L. R. Emmet	2	H. S. Carhart	1
F. Bedell	2	P. Lange	1
S. D. Greene	2	W. D. Weaver	1
A. Macfarlane	2	C. O. Mailloux	1
D. C. Jackson	2	H. Ward Leonard	1
Jos. Wetzler	2	A. Schmidt	1
G. S. Dunn	2	W. J. Hammer	1
C. E. Emery	2	Henry Morton	1
C. T. Hutchinson	2	C. D. Haskins	1

Total.....................975.

FOR MANAGERS.

Total Vote Cast 1290

L. B. Stillwell	288	W. L. R. Emmet	2
J. W. Lieb, Jr	282	W. E. Geyer	2
F. A. Pickernell	282	C. S. Cornell	2
Wm. L. Puffer	274	J. B. Cahoon	2
E. G. Bernard	14	Louis Bell	2
Samuel Sheldon	12	R. Mc. A, Lloyd	1
Wm. Stanley	12	N. W. Perry	1
Wm. S. Barstow	10	C. D. Crandall	1
A. V. Abbott	7	J. S. Brown	1
A. J. Wurts	7	M. K. Eyre	1
G. S. Dunn	5	H. L. Lufkin	1
S. D. Greene	5	E. A. Sperry	1
A. L. Rohrer	5	A. S. Hibbard	1
G. Wilkes	5	W. J. Jenks	1
F. B. Crocker	4	F. J. Sprague	1
Wm. A. Anthony	3	B. F. Thomas	1
O. T. Crosby	3	F. S. Holmes	1
E. C. Davidson	3	C. R. Agnew	1
F. Bedell	3	E. T. Gilliland	1
S. S. Wheeler	3	C. Cuttriss	1
E. W. Rice, Jr	3	W. S. Aldrich	1
J. Wetzler	3	E. Merritt	1
F. S. Pierson	2	A. C. Crehore	1
E. Caldwell	2	H. V. Hayes	1
R. H. Pierce	2	E. Weston	1
A. Dow	2	F. Reckenzaun	1
C. R. Cross	2	D. C. Jackson	1
Wm. Maver, Jr.	2	E. B. Ives	1
F. W. Jones	2	M. I. Pupin	1
C. E. Emery	2	C. J. Field	1
R. B. Owens	2	Nikola Tesla	1
G. D. Shepherdson	2	C. G. Armstrong	1

Total.....................1290.

FOR SECRETARY.

Total Votes Cast..........319.

Ralph W. Pope................319

FOR TREASURER.

Total Votes Cast..............329.

Geo A. Hamilton..............326	W. A. Anthony............... 1
Ralph W. Pope................ 2	

Total....................... 329.

Total number of votes for President (330) indicate total number of ballots counted.

Rejected as not complying with Rules, 28. In all but one case this was due to the omission of name on outer envelope, in the other case tellers could not interpret marks made upon ballot.

Respectfully submitted,
TOWNSEND WOLCOTT,
WM. J. HAMMER,
Committee.

MR WOLCOTT.—I would state that there were 28 ballots rejected because they did not comply with the rules. The envelopes containing 27 of these ballots were received without any signature on the outside by which to identify them, and they had to be rejected. The other ballot was marked in such a way that it was impossible for the tellers to ascertain what the voter meant, and that had to be rejected.

Mr. Pope received 319 votes as Secretary, which was the total number of votes cast. For Treasurer there were 329 votes cast, of which Mr. Hamilton received 326. Although there were more votes cast for Treasurer than there were for Secretary, yet the vote for Treasurer was not unanimous. The reason for that is that some members tore off the Council ticket from their ballots, voting the other part, and the name of Mr. Pope was not on that part.

THE PRESIDENT.—The next business is the report of the Sub-Committee on "Standards of Light."

The following report was then read by Dr. Edward L. Nichols:

A Report presented at the 13th General Meeting of the American Institute of Electrical Engineers, New York, May 20th, 1896.

STANDARDS OF LIGHT.

Preliminary Report of the Sub-Committee of the Institute.

BY EDWARD L. NICHOLS, CLAYTON H. SHARP, AND CHARLES P. MATTHEWS.

One of the sub-committees appointed in 1893 to investigate the subject of a suitable standard of light for photometric purposes has been engaged upon preliminary experiments, and upon the collection of data concerning the existing standards, and also of such other sources of light as might be regarded as possible substitutes for the standards now in use. Such investigation is a necessary preparation for the consideration of recommendations looking to the adoption of any new standard.

The committee is still at work upon these preliminary experiments, but it has reached a stage when it seems desirable to make the following report of progress.

I.

The following sources of light have been in use in photometric work for a sufficient length of time to enable various observers to become acquainted with their merits and likewise with their imperfections.

(1) The Carcel Lamp.
(2) The British Standard Candle.
(3) The German Standard Candle (Vereinskerze).
(4) The Methven Screen.
(5) The Hefner-Alteneck Amyl-acetate Lamp.
(6) The Harcourt Pentane Standard.
(7) " " " Lamp.

In addition to these, the Violle platinum standard of light has been before the scientific public for several years, and although it has not gone into extensive practical use, it has been subjected to severe tests in the laboratory of the German Imperial Institute for Research (Reichsanstalt in Charlottenburg) and elsewhere. Various luminous gas flames, also incandescent lamps, have been extensively used as secondary standards. The following standards have likewise been proposed, and a certain amount of work has been done to demonstrate their good properties and to determine the degree of accuracy with which they can be reproduced.

(1) The crater of the positive carbon in the arc.

(2) The surface of a strip of platinum heated by means of a current to an arbitrarily defined temperature.

Finally there are a number of light sources which must be taken into account in the selection of a standard of light, aside from those which have been mentioned in the above list. Such are gas flames burning within a mantle of pure oxygen (The Bude Light), the acetylene flame, the various incandescent mantle burners, the light from other glowing metallic oxides, such as the zircon light and the light of burning magnesium.

The committee proposes to present in this report a summary of measurements which have been made upon the various sources of light mentioned above, with a view to reaching some decision with reference to their relative merits as light standards. It is hoped thus to pave the way for experiments leading to the recommendation of new definitions of the standard of light, or at least to the recommendation of greatly improved procedure in the handling of existing standards.

In the case of many of these sources of light, the members of the committee have made extensive measurements of their own, either in the verification of existing statements, or in the exploration of questions hitherto not definitely attacked. It is proposed in this report to give a brief resumé of these experiments, some of the results of which have already appeared elsewhere, and to summarize the existing work of previous investigators, so far as the committee is acquainted with the same.

II.

Tests of standards of light belong to one of two general classes; in the first class are included all comparisons of flames with flames, the results of the tests being in this case affected by

the change of the standard flame with the purity, temperature, and hygrometric state of the atmosphere. They show in general only the variations of the standard in question which take place during short periods of time, and demonstrate nothing concerning the variations which take place from day to day. In the second class are included all comparisons of light standards with glow lamps and also bolometer tests.

Tests of British Standard Candles.

The British standard candles were specified by act of Parliament, in 1860, to be sperm candles weighing six to the pound, and burning 120 grains per hour. In spite of universal condemnation by all who have tested this standard, it still maintains its position in Great Britain and America.

Messrs. Harcourt, Keats and Methven, appointed by a committee of the Board of Trade to investigate the performance of British candles, found a difference of 15 per cent. in the average illuminating power of legal candles, while two pairs showed a maximum variation of 22.7 per cent.

Heisch and Hartley, acting for the committee on light standards of the Council of the Gas Institute, found that the differences in the illuminating power of candles ranged from 1.3 per cent. to 16 per cent., the average difference being 7.05 per cent. They also reached the conclusion that sperm candles developed more light per grain of sperm consumed, than they had done several years before.

Dibdin conducted two long series of experiments with various standards, reporting the results of the tests to the Metropolitan Board of Works. He compared the standards with the flame produced by a specially stored coal-gas. His tests were very extensive, and his reports voluminous and exhaustive. For the British candle he found in his first series of tests a maximum variation of 14.9 per cent., which occurred twice, and a maximum total variation in a single group of 23.2 per cent. which also occurred twice. The mean variation, disregarding signs, was 3.6 per cent. Variations of from five to eight per cent. were common; 13.7 per cent. of the tests were within one per cent. of the mean. In the second series the maximum variation was 11.7 per cent., and the maximum total for one group 19.3 per cent. The mean variation was smaller on account of the fewer observations in each group. This may account for the

fact that in this series 34 per cent. were within one per cent. of the mean.

In his next report he combats the view of Heisch and Hartley that candles gave more light per grain of sperm then, than they had given several years before. Comparing determinations of the candle power of the Carcel lamp made in 1870 with others made in 1879, he found that the illuminating power of candles had decreased rather than increased during these nine years.

A committee of the British Association, comprising numerous distinguished members of that body, in their fourth report rendered at the Plymouth meeting in 1888, gave the results of extensive tests of candles. Their comparison standard was a burner supplied with coal-gas which had been enriched with pentane. Of 118 experiments, 98 gave differences of one per cent. from the mean, 57 gave differences of two per cent., 19 of five per cent. while differences of nine per cent. to ten per cent. were produced only very irregularly. They concluded that candles are not worthy to be called standards, although they conform to the legal requirements, and that the intensity of their light is affected by the purity of the air in the room, the shape and construction of the wick, the nature of the sperm and by other causes.

They also pointed out that the spermaceti is not a substance of definite chemical composition; that improvements in the process of manufacturing have resulted in what is known as a "dryer" sperm, one containing less oil; that to prevent crystallization, a variable quantity of beeswax is added. They inferred from these considerations that it is probably true that the illuminating power of candles has changed since the quality of sperm employed, the construction of the wick with respect to the number of strands, the tightness of the twist, etc., are not specified in the act, but are left to the option of the makers. This committee regarded as the chief source of the oscillations of the light:

(1) Changes in the length and shape of the wick.

(2) Difference in the height of the melted sperm in the cup of the candle.

Photometric observations by the many other observers have simply served to confirm the above conclusions. A recent

Dutch commission, for example[1], found, from many tests, the mean fluctuation in the intensity of British standard candles to be ± 2.43 per cent., with a maximum of 9.70 per cent.

Methven has shown the following variations in the intensity of a candle to take place with changes in the azimuth of the plane of the wick. Two candles were used, their intensities being as follows:

Plane of the wicks perpendicular to bar, C. P. = 1.999.

Wicks pointing away from the photometer, C. P. = 1.933.

Wicks pointing towards the photometer, C. P. = 1.957.

He found also that a candle which gave in dry air an intensity of 1.104 gave in moist air an intensity of 1.196,—a variation of 8.38 per cent.[2]

III.

Bolometric Measurement.[3]

The extraordinary fluctuations of such sources as the British candle, make photometric measurements difficult and uncertain. The consideration of the discrepancies exhibited in the results of previous observers suggested to the members of your committee the substitution of the bolometer for the eye, in the study of such sources of light. The following is a summary of the investigation which resulted from this suggestion.

Description of Apparatus.

A piece of Swedish iron wire, of No. 30 B. & S. gauge, was passed through jeweler's rollers until its thickness was about 0.045 mm. and its width 1.5 mm. It was then placed in dilute sulphuric acid, in which potassium bichromate had been dissolved, and a current was passed through it in such a manner as to dissolve the iron. The potassium bichromate was introduced into the solution to dispose of the hydrogen bubbles which would ordinarily have clung to the metallic surface, and which would have caused it to be dissolved unevenly.

1. For an abstract by Krüss, see *Journal für Gas Beleuchtung und Wasserversorgung* (1894).

2. John Methven : *Dingler's Polytechnisches Journal*, vol. 277, p. 276, taken from London *Gas World*, 1889, p. 572. See also Sugg : *Journal for Gas Lighting*, which is reprinted in the *Scientific American Supplement*, No. 484, p. 7726.

3. For a more detailed description of these measurements, see Sharp and Turnbull, *Physical Review*, vol. ii, p. 1.

In this way the strip was obtained which was about 0.025 mm. in thickness and still moderately strong. From this strip were cut two pieces, each about 6 cm. in length, to constitute two arms of a Wheatstone bridge.

To carry the strips so obtained, a light oblong frame, *F* (Fig. 1), of thin wood was made, and to it were fastened small bits of sheet brass, *b*, *b*, *b*, to which the strips and the copper wires intended to connect them with the other arms of the bridge could be soldered. The strips, *S*, *S'*, were then bent and placed over the frame, so that each strip crossed the frame twice. The free ends of each strip were displaced laterally from each other, so that, when viewed from the front, the portion of the strip on one side of the frame hid only very little of the portion on the other side of the frame.

After the two strips had been arranged on the frame sym-

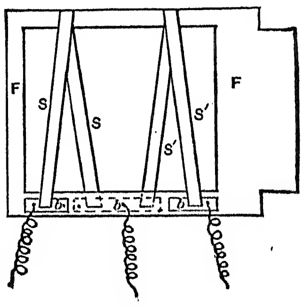


Fig. 1.

metrically with respect to each other, the one which was to receive radiation was carefully smoked on both sides. To accomplish this smoking without undue heating of the strip, a piece of sheet metal, through which a small hole had been punched, was held over a candle flame so that the flame was caused to smoke. The smoke passed through the hole, over which a tube was held to direct the current. The strip was passed back and forth over the top of the tube. In this way a very delicate strip can be blackened without injury. In their completed state, the strips had a resistance of about 0.5 ohm each.

The frame holding the strips was mounted in a wooden box about 20 cm. long and 5 cm. × 6 cm. in cross-section. A number of suitable cardboard screens were placed in the front portion of the box to shield one of the strips from radiation and to protect both of them from draughts. To close the box at its

other end, a piece of bright tin was cut equal in size to the cross-section of the box. This was soldered fast to a heavy block of brass, to the other end of which was soldered another piece of tin which covered the end of the box. When this arrangement was placed in the box, the first piece of tin came close behind the bolometer strips, and its bright surface acted as a mirror in reflecting upon the back of the exposed strips many rays which would otherwise have been lost.

By this means the efficiency of the bolometer was nearly doubled. The tinned surface did not tarnish perceptibly during the course of the investigation. It was not smooth enough to reflect a distinct image, and the light reflected from it was to a large extent scattered. The use of a plane-surfaced mirror in such a position would not be allowable, since any slight change in the angle of incidence would cause a different amount of light to be reflected upon the bolometer strip. The use of the irregular-surfaced plate, since it diffuses the light, can scarcely affect the accuracy of the results to an appreciable degree in such work as has been done with this bolometer. Nevertheless, this arrangement is to be recommended only where great sensitiveness is desired rather than the most exact comparison of results.

The galvanometer employed was of the four-coil type. It was constructed by Professor W. S. Franklin, after the same general plan as has been followed by Snow,[1] Paschen,[2] and others. When the two front coils were in multiple with each other and the two rear coils similarly connected, and the two pairs were connected in series, the resistance was found to be 190 ohms. The moving parts consisted of four little magnets of piano wire, each about 5 mm. long, and a mirror of thin cover glass, 4 mm. wide by 7 mm. long, all mounted on a slender rod of glass and suspended by a very fine quartz fiber. Any oscillations of the needles were very strongly and effectively damped by the air resistance to the light-moving parts—a very essential condition to the correct operation of the instrument when used to get the variations of a rapidly fluctuating source of radiant energy.

The scale was divided into 100 half-inch divisions, each of

1. B. W. Snow, "Wied. Ann.," Vol. 47, p. 213; *Phys. Review*, Vol. 1, p. 2.

2. F. Paschem, "Wied. Ann.," Vol. 48, p. 272; *Zeitschr. für Instrumentenkunde*, Vol. 13, 1893, p. 13.

3. Ångström, "Oefvers. af kongl. Vetenskaps-Akad. Förhandl.," 1888, Vol. 6, p. 379.

which was, in turn, divided into tenths. The distance of the scale from the galvanometer was 100 scale divisions, *i. e.* 50 inches. With the telescope used, fifths of the smallest divisions could be estimated. In speaking of scale divisions, the half-inch divisions will always be meant.

To test the sensitiveness of the apparatus, the galvanometer was adjusted until the period of the needles was six seconds for a complete vibration. A deflection of one scale division corresponded to a current of 68×10^{-10} amperes.

This deflection of one scale-division corresponded to a temperature rise in the bolometer strips of $0^\circ.00657$ C. If we reduce this to millimeter divisions on a scale placed at a distance of 1 m. from the mirror, we see that one millimeter deflection corresponded to a current of 68×10^{-11} amperes, and the corresponding rise in temperature of the strip was $0^\circ.00066$ C.

This temperature sensitiveness is much smaller than has usually been employed in bolometer work, but it was amply sufficient for the purpose. That the bolometer itself was one of high sensitiveness is evident from the fact that this degree of sensitiveness was attained with galvanometer needles swinging in a strengthened field and with a galvanometer of 190 ohms resistance. The conditions for maximum sensitiveness of the bridge would have required a galvanometer resistance of only 0.5 ohm.

The reason for the great sensitiveness lay in the nature of the strips employed. Their area was considerable, the temperature coefficient of the iron was high, and the current through it was large, ranging from 0.15 to 0.20 ampere, the size of the strips permitting the use of a large current without undue heating. As a result of the strong field in which the galvanometer needles swung, the drift due to magnetic changes was usually imperceptible.

The bolometer, compensating resistance, and battery were all placed in an interior room, with thick brick walls, and having communication with the outer room only by a door, *D*, Fig. 2.

The temperature of this room changed very slowly, and it was quite free from drafts. The bolometer box, *B*, was placed upon a shelf fastened to the door of the room, and looked out through a hole in the door upon the outer room. A double screen, *S*, of tin, arranged to slide up and down on the outside of this door, covered up the bolometer strips when desired. The box *B'*, containing the compensating resistance, was also fastened

to the inside of the door. The end of the box through which the screw projected, fitted into a hole in the door, so that the screw would be turned and the bridge balanced from the outer room.

In getting the variations of candles, a special device was required to keep the top of the burning candle at constant height in front of the bolometer strip. A suggestion of Mr. C. H. Bierbaum, M. E., resulted in the construction of the following simple and effective arrangement. A spiral spring, about 60 cm. long and 4 cm. in diameter, was attached to a small scale-pan,

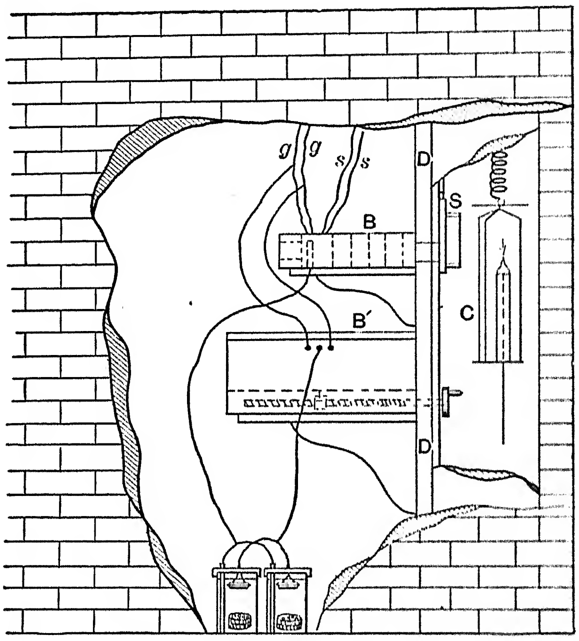


FIG. 2.

and the spring was cut off to such a length that when a candle was put on the scale-pan, the elongation was just equal to the length of the candle. The spring would then take up as fast as the top of the candle was lowered by burning. A small piece of sheet metal served to protect the spring from the heat of the candle. In order to keep the scale-pan from swinging sidewise and from oscillating up and down, a couple of wires were passed vertically through holes on opposite sides of it, and served as loosely fitting guides, which, without interfering with the take-up of the spring, effectually damped any vibrations. The ad-

justment of a spring to any candle could be made in a few minutes with sufficient accuracy so that the height of the top of a candle burning on the scale-pan would not vary over 1 mm. in an hour. *C*, Fig. 2, shows this arrangement in place.

Method of Taking Observations.

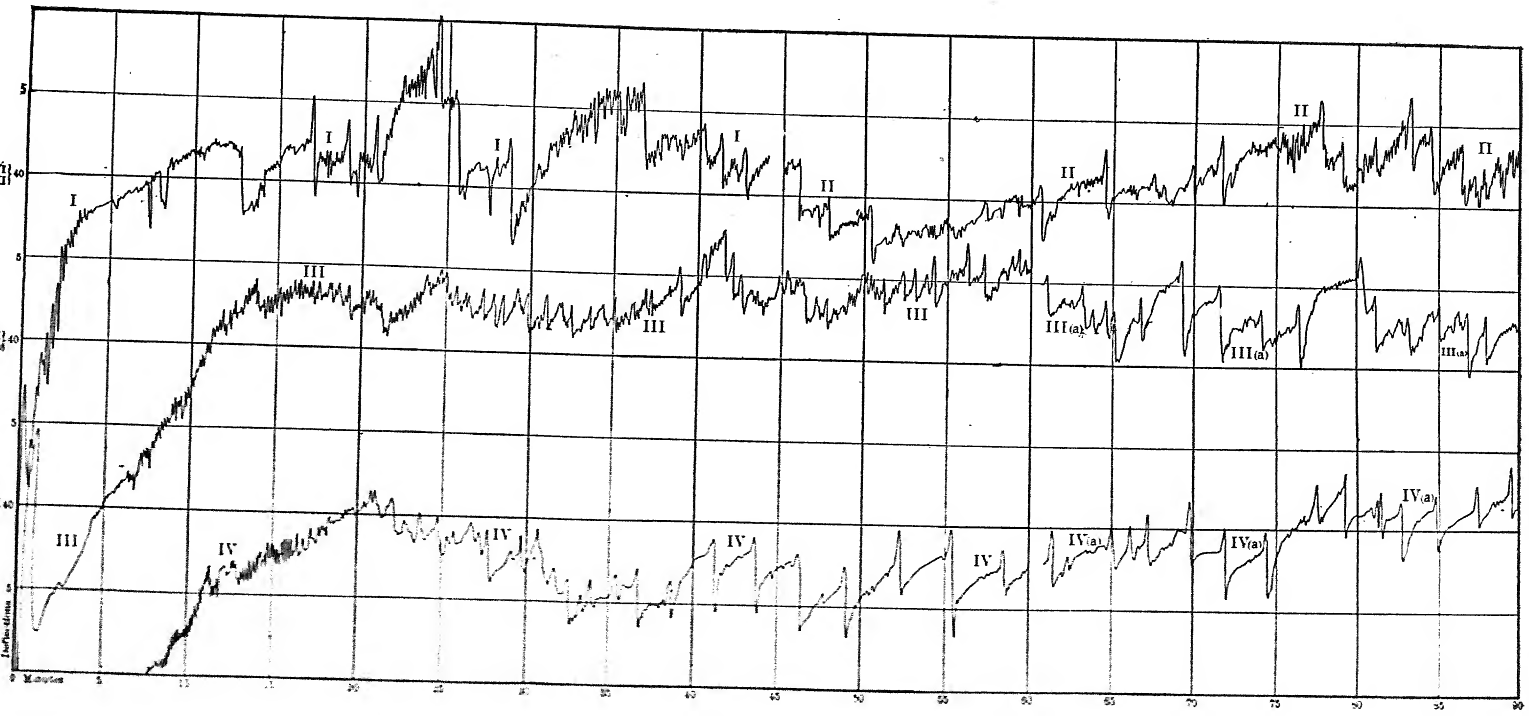
All determinations of the variations of standards were made at times when the laboratory and its surroundings were very quiet. Most of them were made between the hours of seven and twelve in the evening; a few were made on holidays, when the laboratory was closed for general work. Before beginning a set of observations the sensitiveness was adjusted, and the galvanometer was carefully watched for a considerable time to get the amount of its swings due to currents of air about the bolometer strips or to changes in the earth's field. The sensitiveness was also tested from time to time. If the movement amounted to more than two tenths of a scale division no run was attempted.

After taking these preliminary observations which usually required about an hour's time, the bolometer was exposed to the source of light. The galvanometer deflections were read rapidly by one person and were plotted by another as fast as read, the times being taken from a watch. In this way curves were traced which represent very truly all the changes in the radiation of the light source.

The character of the results obtained with the British candle is exhibited in Fig. 3.

An unfortunate feature in these curves is that the scale of abscissas, representing times, is so small. As a result of this, it has been difficult to represent with the greatest accuracy the true slope of those portions of the curves which correspond to very rapid changes in radiation. In examining the curve, it should be borne in mind that a very steep line may cover several seconds of time, and that it is quite possible that a photometric setting might be made during the time that a candle is executing just such a variation as is represented by the steepest parts of one of the candle curves. The number of galvanometer readings plotted during the space of five minutes was usually from 50 to 100.

At the end of a run, the strips and surrounding parts were given time completely to lose the heat imparted to them, after which readings for zero and sensitiveness were taken. The



I
I
I
I
II
II
II
II
III
III
III
III(a)
III(a)
III(a)
III
IV
IV
IV
IV
IV(a)
IV(a)
IV(a)
Deflections
Minutes

FIG. 3.

curves were corrected for changes in zero and in sensitiveness, using the equation :—

$$\frac{\text{change in sensitiveness}}{\text{initial sensitiveness}} \times \text{approximate mean ordinate} = \text{correction to ordinate.}$$

It has been impossible to draw the curves so as to show clearly the minor fluctuations due to minute changes in the intensity of the radiation, since, as has been said, the scale to which the time abscissas are plotted is very small, compared with the scale of ordinates. These important peculiarities of the movements of the galvanometer needle were clearly perceptible, however, to one who observed the galvanometer deflections for only a litte time, while their character made it perfectly evident that the swings of the galvanometer needles in their natural period exercised only an inappreciable influence on the slopes of the curves and the magnitudes of the variations recorded.

TABLE I.

BOLOMETRIC MEASUREMENS OF THE BRITISH CANDLE.

(DATA FOR CORRECTIONS OF THE CURVES.)

Number of curve.	Times of taking zero and sensitiveness readings.	Time of lighting.	Time of taking the curve.	Hourly rate of consumption.	Distance from bolometer.	Initial sensitiveness.	Correction for		Reduction to standard		
							Drift.	Change of sensitiveness.	Sensitiveness.	Distance.	Rate.
	h. m. h. m.	h. m.	h. m. h. m.	grams	cm.						
I.	8 47 and 10 01	8 50	8 50 to 9 35	7.910	25.3	16.2	+0.9	+0.2	0	−2.3	−0.70
II.	9 41 10 50	9 33	9 50 10 35	7.767	26.0	16.1	−0.4	0	+0.3	0	+0.05
III.	8 00 9 15	8 00	8 00 9 00	7.842	26.0	16.1	−2.9	+0.8	+0.3	0	−0.34
III. (*a*)	9 38 10 30	8 00	9 45 10 15	7.842	26.0	16.08	+0.3	0	+0.3	0	−0.34
IV.	7 50 9 05	7 50	7 50 8 50	7.692	25.3	16.15	+1.0	−0.1	+0.1	−2.1	+0.45
IV. (*a*)	9 35 10 30	7 50	9 45 10 15	7.692	25.3	16.4	−0.1	0	−0.5	−2.3	+0.45

In Fig. 3, and in subsequent cuts containing bolometric curves, the smallest divisions of abscissas represent five minutes of time. The corresponding divisions of ordinates are five divisions of the galvanometer scale. In order to economize space, no attention has been given to the height of the curves relative to the base line on the plates. The total ordinates for each curve are indicated by such a symbol as this $\left.\begin{matrix}\text{I}\\\text{II}\end{matrix}\right\}$ 40, indicat-

ing that the true X-axis for curves I. and II. should be placed ten divisions below the base line on the plate. Consequently, on the various plates, curves having the same ordinates are plotted one above the other, the true ordinate being indicated in the way mentioned above.

The data by which the curves have been corrected are given in Table I.

Table II. gives, in its first column, the approximate abscissas of the portion of the curve in question, and in its second column the mean ordinates of these portions. The third column contains the deviations of the mean ordinates of the portions from the mean ordinate of the whole curve. The fourth column contains these deviations reduced to percentages of the whole. The fifth contains similar deviations from the mean ordinate of all the curves of the particular standard under consideration, and in the sixth these also are reduced to percentages.

Discussion of the Results Obtained with British Candles.

Fig. 3 shows the curves given by the English standard candles. In curves III. and IV. the candles were lighted at their tops. The wicks flared up and gave a high point on each curve, which in the case of curve IV. is not shown on the plate. The flames then increased gradually to their normal size, which was reached after about 15 minutes. Curves III. (*a*) and IV. (*a*) are continuations of III. and IV. The candles were allowed to burn during the interval between the curves, which in the case of III. and III. (*a*) was 45 minutes, and in the case of IV. and IV. (*a*) was 55 minutes. During III. the room was rather more draughty than during III. (*a*), and the effect of the draughts is seen in the much larger numbers of small irregularities in the former than in the latter curve. During the interval between IV. and IV. (*a*) the height of the flame of the candle was found to vary between 43 and 48 mm.

Curve I. was taken with the portion of candle left over from IV. and IV. (*a*). It was lighted, its wick being already charred and its crater formed, and readings were taken immediately. Curve II. was taken with the lower half of the candle used in getting III. and III. (*a*). The bottom, *i. e.* the larger end of this, was hollowed out to expose the wick, and readings were taken after the candle had been burning long enough to come to its normal light-giving power. The agreement in the amount of

TABLE II.

RESULTS OF BOLOMETRIC MEASUREMENTS (BRITISH CANDLE).

	Time: Minutes on curve.	Mean ordinates and their mean.	Deviations from the mean ordinate of the curve.	Deviations reduced to percentages.	Deviations from mean ordinate of all the English candle curves.	Deviations from general mean reduced to percentages.
CURVE I.	15–20	41.68	−10.4	−2.43	+0.59	+1.44
	20–25	44.68	+1.96	+4.58	+3.59	+8.72
	25–30	40.84	−1 88	−4.40	−0.25	−0.61
	30–35	43.39	+0.67	+1.57	+2.30	+5.59
	35–40	43.71	+0.99	+2.32	+2.62	+6.37
	40–45	42.04	−0.68	−1.59	+0.95	+2.31
		42.72	Correction for rate of burning = −0.70.			
CURVE II.	45–50	39.24	−1.77	−4.32	−1.85	−4.50
	50–55	37.85	−3.16	−7.70	−3 24	−7.87
	55–60	39.12	−1.89	−4.61	−1.97	−4.79
	60–65	40.44	−0.57	−1.39	−9.65	−1.58
	65–70	40.76	−0.25	−0.61	−0.33	−0.80
	70–75	42.85	+1.84	+4.48	+1.76	+4 28
	75–80	43 52	+2.51	+6 12	+2.43	+5 92
	80–85	43.20	+2.19	+5.34	+2.11	+5.13
	85–90	42.11	+1.10	+2.68	+1.02	+2.48
		41.01	Correction for rate of burning = +0.05.			
CURVE III.	15–20	43.04	−0.54	−1.25	+1 95	+4.74
	20–25	42.88	−0.70	−1.62	+1.79	+4.35
	25–30	42.82	−0.76	−1.76	+1.73	+4.21
	30–35	42.18	−1.40	−3.23	+1.09	+2.65
	35–40	43.23	−0.35	−0.80	+2.14	+5.20
	40–45	44.80	+1.22	+2.80	+3.71	+9.02
	45–50	43.58	0	0	+2.49	+6.06
	50–55	44.30	+0.72	+1.65	+3.21	+7.80
	55–60	45.38	+1.80	+4.13	+4.29	+10.45
		43.58	Correction for rate of burning = −0.34.			
CURVE III. (a).	60–65	43 25	+0.23	+0.53	+2.16	+5.25
	65–70	43.52	+0.50	+1.16	+2.43	+5.91
	70–75	42.82	−0.20	−0.47	+1.73	+4.21
	75–80	43.68	+0.66	+1.54	+2.59	+6.30
	80–85	42.82	−0.20	−0.47	+1.73	+4.21
	85–90	42.04	−0.98	−2.28	+0.95	+2.31
		43.02	Correction for rare of burning = −0.34.			
CURVE IV.	15 20	38.65	+1.42	+3.82	−2.44	−5.94
	20–25	39.84	+2.61	+7.01	−1.25	−3.04
	25–30	38 07	+0.84	+2.26	−3.02	−7.35
	30 35	35.76	−1.47	−3.87	−5.33	−12.95
	35–40	35.72	−1.51	−4.05	−5.37	−13.08
	40–45	37.04	−0.19	−0.51	−4.05	−9.84
	45–50	35.84	−1.39	−3.75	−5.25	−12.78
	50–55	37.16	−0.07	−0.02	−3.93	−9 55
	55–60	36.96	−0.27	−0.72	− 4.13	−10.05
		37.23	Correction for rate of burning = +0 45.			
CURVE IV. (a).	60–65	38.26	−1.47	−3.70	−2.83	−6.88
	65–70	39.04	−0.69	−1.74	−2 05	−4.99
	70–75	38 26	−1.47	−3.70	−2.83	−6.88
	75–80	40.84	+1.11	+2.80	−0.25	−0.62
	80–85	40.84	+1.11	+2.80	−0.25	−0.62
	85–09	41.16	+1.43	+3.60	+0.07	+0.17
		39.73	Correction for rate of burning = +0.45.			

Mean ordinate of all the English candle curves is 41.09.
Mean ordinate of all the English candle curves corrected for rate of burning is 41.05.
Mean ordinate of all the English candle curves corrected for rate and reduced to true deflections is 41.06.

·adiation of the candle burned in this way, with the amount vhen burned from the smaller end, shows that the variation in :he diameter of the candle has little if any influence on the in- :ensity of the light emitted.

One marked peculiarity which characterizes, to a greater or less degree, all these curves, is the succession of sudden drops followed by gradual rises to a maximum. In the case of the drop in curve IV. at 55 minutes, the change amounted to 15 per cent. of the total deflection, and in other instances the change was nearly or quite as large. The reason for these drops is to be looked for in the action of the wick, which, as the candle burns down projects farther above the spermaceti, causing a tall flame. Finally, by reason of charring and because of its own weight, it bends over and the ends burns off. The flame following the wick becomes shorter.

Since the wicks of English standard candles are very uniform in construction, these drops succeeded each other after nearly regular time intervals of about three minutes.

A confirmation of these results, together with conclusive evidence of the legitimacy of the bolometric method of studying light sources will be found in section IV. of this report.

IV.

The study of the irregularities exhibited by the British candle suggested to one of the members of your committee that the use of a different criterion of the light emitted by a candle than that now employed, namely, rate of consumption of sperm, might lead to better results in their use. The method proposed is to measure at the time that the photometric setting is made, the height of the flame. Then knowing the relation between flame height and intensity of light emitted, to reduce the instantaneous intensity to intensity at standard height.

Two methods were employed to determine the ratio of the flame height to the intensity. In the first method the intensity of the radiation of the candle was measured by the bolometer in the way previously described. To measure the height of the flame, a long camera was constructed, having its lens and ground glass plate at a fixed distance from each other. The ground glass plate was graduated empirically to read directly in millimeters the height of objects focussed upon it. The candle was placed on a pan attached to a spiral spring of such a length that

its elongation would be just equal to the length of the candle. By this arrangement the top of the candle was kept at a constant height above the floor, and when once the image of the base of the flame had been accurately adjusted on one of the lines of the camera screen (to facilitate which the screen was capable of a slight vertical movement), it remained there during a considerable period of time. Hence, to measure the height of the flame at any instant, it was necessary only to glance at the image of the *top* of the flame and to note its position on the screen.

The magnifying power employed was about two, and heights were measured only to 0.5 mm. Greater closeness of measurement was deemed unnecessary on account of the ill-defined nature of the base and tip of the flame. The actual base of a candle flame is difficult to observe, on account of the small quantity of light which it emits. The procedure adopted was to set on the line of demarkation between the charred and uncharred portions of the wick, since this was found usually to mark the base of the flame. In case a close inspection of the candle showed the base of the flame to be slightly above this point, a further adjustment of the screen was made.

In order to ensure great steadiness of the flame, the candle was placed in a roomy, well ventilated box having a glass window. Having put the candle in position before the bolometer, and having adjusted the camera properly, the bolometer screen was raised, and simultaneous observations were made of galvanometer deflections and flame heights. These readings were corrected for any change in sensitiveness of the bolometer and any drift of the galvanometer needle, and were plotted, using flame heights and galvanometer deflections as co-ordinates.

In the second method, a Lummer Brodhun photometer was used. At one end of a photometer bar 200 in. long was placed a 110-volt glow lamp. This was maintained at a voltage of 100 by means of a storage battery. Being run at a low efficiency, its color was about the same as that of a candle, and its change in candle power during the time it was in use was too small to be detected.

At the other end of the bar was the candle, supported by its adjusted spring. The candle was always placed so that the curl

of the wick was perpendicular to the axis of the bar.[1] The arrangement for measuring flame heights is shown in Fig. 4, which represents a projection of the apparatus on a horizontal plane. *B* is the photometer bar, *C* the candle, *M* a mirror placed behind the candle in such a way as to reflect the rays from it through the lens *L*, which projected them on a graduated screen, *S*, placed immediately behind the bar. The mirror was carried on a movable support, so as to admit of an adjustment of focus.

This arrangement was adopted, since by its use one observer could do all the work. The method was simply to make a rather quick photometer setting, and then instantly to note the position of the top of the flame on the screen, reading the position of the photometer afterwards.

The observations were treated in the following manner: The various observed values of flame height were collected in such a way that heights of 41.0 mm., and 41.5 mm., and 42.0 mm.,

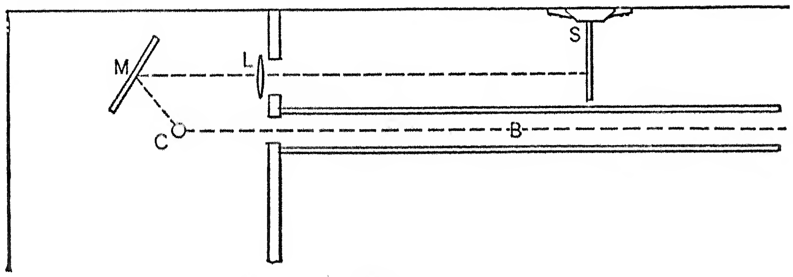


Fig. 4.

formed one group; 42.5 mm., 43.0 mm., and 43.5 mm., another, etc. The mean height of each group was found, and also the mean bar-reading corresponding to it. The candle power of the standard was found by taking the mean of all the heights and bar-readings, and reducing, by means of an approximate correction, to a standard height flame of 45 mm. Using this value for the intensity of the glow lamp, the intensity of the candle corresponding to each group was computed. By means of a curve plotted from these values, the percentage variation per millimeter of flame height was determined.

In order to find the relative accuracy of this, and of the ordi-

1. For the variation of the intensity of candle as a function of the azimuth of the plane of the wick, see Methven, London *Gas World*, 1889, p. 572.; also an article by Sugg, in the *Journal for Gas Lighting*, which is reprinted in the *Scientific American Supplement*, No. 484, p. 7726.

nary method of using candles, the following observations were made: A candle burning normally was weighed by the "method of transits," was transferred to the spring balance, and ten or more photometer settings made—the flame height also

TABLE III.

Flame height, mm.	Galvanometer deflection.	Flame height, mm.	Galvanometer deflection.	Flame height, mm.	Galvanometer deflection.
48.0	29.0	44.0	[illegible]	[illegible]	[illegible]
48.5	29.5	45.0	[illegible]	[illegible]	[illegible]
49.5	30.3	45.5	[illegible]	[illegible]	[illegible]
48.0	29.2	43.5	[illegible]	[illegible]	[illegible]
47.0	28.2	43.5	[illegible]	[illegible]	[illegible]
47.5	28.8	44.5	[illegible]	[illegible]	[illegible]
45.5	27.5	44.5	[illegible]	[illegible]	[illegible]
46.5	28.0	45.0	[illegible]	[illegible]	[illegible]
46.5	28.2	44.5	[illegible]	[illegible]	[illegible]
46.5	28.0	43.5	[illegible]	[illegible]	[illegible]
45.0	27.0	43.5	[illegible]	[illegible]	[illegible]
45.5	27.6	43.0	[illegible]	[illegible]	[illegible]
45.5	27.8	42.5	[illegible]	[illegible]	[illegible]
44.5	26.5	43.5	[illegible]	[illegible]	[illegible]
42.0	25.8	42.5	[illegible]	[illegible]	[illegible]
43.0	26.8	43.5	[illegible]	[illegible]	[illegible]
43.0	26.0	44.0	[illegible]	[illegible]	[illegible]
43.5	26.7	43.5	[illegible]	[illegible]	[illegible]
44.0	26.8	43.5	[illegible]	[illegible]	[illegible]
44.5	27.1	44.0	[illegible]	[illegible]	[illegible]
44.5	27.2	44.5	[illegible]	[illegible]	[illegible]
43.5	26.4	44.0	[illegible]	[illegible]	[illegible]

TABLE IV.

Flame height, mm.	Photometer bar.	Flame height, mm.	Photometer bar.	Flame height, mm.	Photometer bar.
45.5	702	41.5	714	[illegible]	[illegible]
46.5	699	43.0	707	[illegible]	[illegible]
48.0	700	43.0	708	[illegible]	[illegible]
49.0	693	44.0	[illegible]	[illegible]	[illegible]
51.5	690	44.0	707	[illegible]	[illegible]
53.0	697	45.0	[illegible]	[illegible]	[illegible]
44.5	709	44.0	[illegible]	[illegible]	[illegible]
46.0	705	44.0	710	[illegible]	[illegible]
49.0	694	43.0	711	[illegible]	[illegible]
46.0	699	45.0	708	[illegible]	[illegible]
48.0	698	End of	wick	[illegible]	[illegible]
48.0	693	cut	off.	[illegible]	[illegible]
49.0	691	40.0	712	[illegible]	[illegible]
45.5	700	44.0	708	[illegible]	[illegible]
44.0	707	47.0	698	[illegible]	[illegible]
45.5	699	48.0	696	[illegible]	[illegible]
48.0	697	50.0	[illegible]	[illegible]	[illegible]
46.0	698	54.0	688	[illegible]	[illegible]
46.5	696	53.0	688	[illegible]	[illegible]
45.0	700	51.0	[illegible]	[illegible]	[illegible]
45.0	701	50.0	694		

being noted. The candle was then weighed again, and another group taken. A number of such sets of observations were made on several days, and since the glow lamp was used as a reference standard, these sets are comparable with each other.

The mean value of the candle power of the glow lamp given by each of these sets of observations was corrected for rate in the ordinary way, and also corrected by reducing from the mean

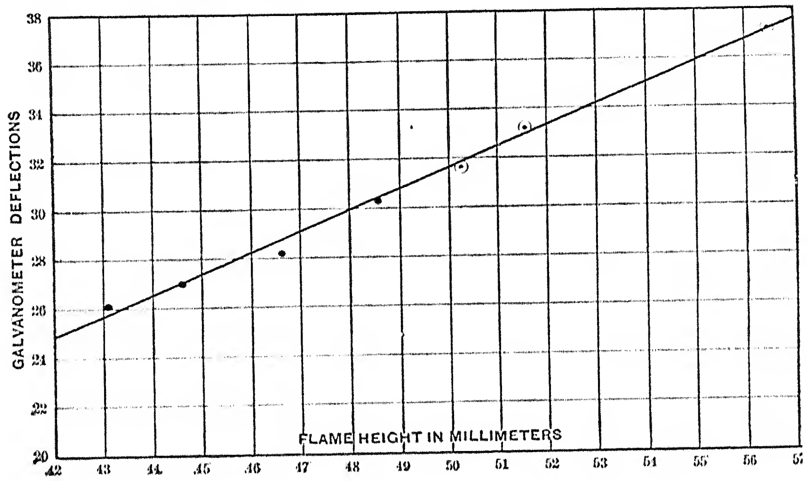


Fig. 5.

flame height to the standard height of 45 mm., using the mean value of the relation between intensity and flame height as determined from all the observations, both bolometric and photometric. The deviation of each value obtained for the candle power of the glow lamp from the mean value obtained from all the observations was computed, and this deviation was reduced to percentages. A comparison of the percentage deviations given by the two methods shows their relative accuracy, while the absolute values of the percentages show the error which one is liable to make in using candles in either of the two ways.

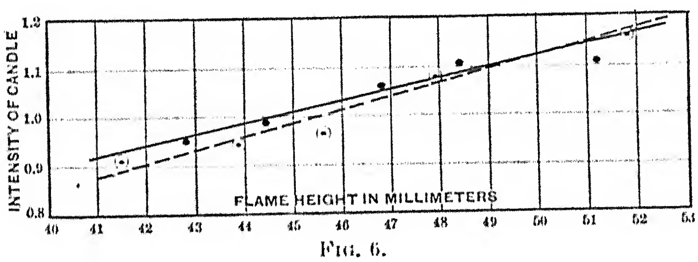


Fig. 6.

Table III. gives a characteristic set of data obtained by the use of the bolometer, 1894, March 21. Table IV. is a similar set of photometric data obtained 1895, November 6. The data of

Table III. and Table IV. are shown reduced and plotted in Fig. 5 and Fig. 6 respectively. Table V. gives values found for the variation in intensity of candles expressed in percentages per milli-

TABLE V.

Bolometric.	Per cent.	Photometric.	Per cent.	Photometric.	Per cent.
1894. Nov. 29 . . .	2.2	1895. July 27 . . .	3.3	1895. Dec. 3 E	
Dec. 5 . . .	2.7	Oct. 30 . . .	3.4	G	2.3
1895. Feb. 23, I.	2.2	Nov. 1 I.	3.3	H	
II.	2.5	II.	3.3	Dec. 4 I	
III.	3.2	Nov. 6 I.	2.2	J	
March 21, I., II.	3.1	II.	2.7	K	2.6
Oct. 4	2.7	Nov. 27 I.	2.7	L	
		II.	2.9	M	
		Dec. 2 A.M., A, B	2.7	N	
		Dec. 2 P.M., C, D, D'	2.8		
Mean . . .	2.7			Weight mean of all	2.7

TABLE VI.

Name of group of observations.	Rate.	Flame height.	C. P. of glow lamp uncorrected.	C. P. corrected for rate.	C. P. corrected for flame height.	Deviation from mean corrected for rate.	Deviation from mean corrected for height.	Percentage deviation. Rate.	Percentage deviation. Height.
								0/0	0/0
A	7.601	43.0	6.11	5.99	5.78	+ 0.35	+ 0.04	+ 0.0	+ 0.7
B	8.320	45.15	5.785	6.20	5.80	+ 0.56	+ 0.06	+ 9.8	1.0
C	6.981	44.95	6.01	5.41	6.00	− 0.23	+ 0.26	− 4.25	+ 4.5
D	8.100	46.1	6.09	6.35	6.25	+ 0.71	+ 0.51	+ 12.6	+ 8.7
E	7.157	46.4	5.52	5.09	5.73	− 0.55	− 0.01	9.8	0.2
F	7.863	44.7	5.67	5.74	5.62	+ 0.10	− 0.12	+ 1.6	2.1
G	8.030	44.8	5.72	5.91	5.60	+ 0.27	− 0.06	+ 4.8	0.0
H	7.330	44.05	5.80	5.47	5.66	+ 0.17	− 0.08	+ 3.0	1.4
I	8.422	44.3	5.65	6.13	5.54	+ 0.49	− 0.20	+ 8.5	3.5
J	7.271	47.1	5.35	5.02	5.65	− 0.62	− 0.09	11.0	1.6
K	7.367	46.15	5.50	5.22	5.67	− 0.42	− 0.07	7.6	1.2
L	7.440	45.6	5.64	5.41	5.73	− 0.23	− 0.01	4.25	0.2
M	7.137	42.3	6.00	5.52	5.56	− 0.12	0.18	− 2.3	− 1.2
N	7.301	44.2	5.79	5.45	5.67	− 0.19	0.07	3.5	1.2
Means, disregarding signs....	7.594	44.9	5.76	5.64	5.74	0.36	0.126	6.5	2.16
Nov. 6, I.		45.5	5.60		5.68		0.09		1.6
II.		45.3	5.79		6.00		+ 0.23		+ 4.0
Nov. 11		43.1	6.17		5.86		0.08		1.4
Nov. 12, I.		44.1	5.87		5.71		0.04		0.7
II.		42.9	6.20		5.86		0.08		1.4
Dec. 2, D'		49.3	5.36		5.90		+ 0.22		+ 4.8
Means of all, disregarding signs.........		45.0	5.78		5.77	Last six only.	0.123		2.14

meter. Table IV. shows, in the way described above, the comparative accuracy of the two methods of reducing candle observations. Fig. 7 shows plots obtained from groups of observa-

tions designated by A, B, C, ... N. Fig. 8 shows plots from three successive sets of bolometric observations.

An inspection of the tables and curves will show that while

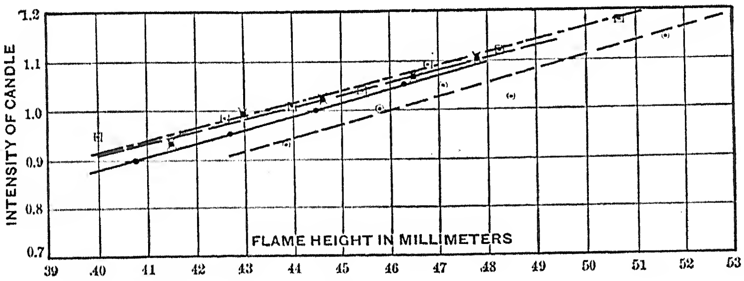


FIG. 7.

the relation between flame height and intensity is a fairly definite one for any given group of observations, there is a considerable range of variation in the values for it as obtained from different groups. Moreover, the relation sometimes changes during one burning of the candle. Fig. 8 illustrates this peculiarity in, that there is a group of points marked [·] which lie considerably below the line plotted to represent all the observations; and that

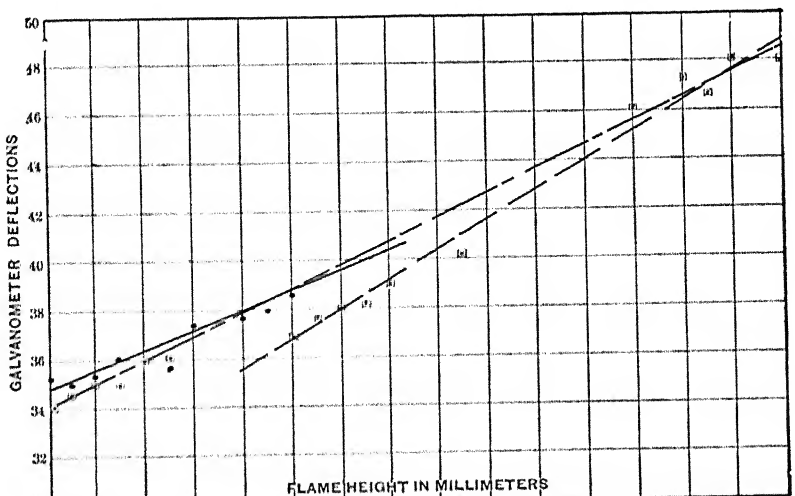


FIG. 8.

the slope of the flame height-intensity curve plotted from the points marked [·] is materially different from that obtained from other groups of points. This may be due to a change in

the shape of the wick. The fact that this relation is not absolutely constant does not vitiate the proposed method of treating candle observations; for since the deviations of flame height from 45 mm. is seldom more than 10 per cent. if our assumed value for this relation is in error by as much as 20 per cent. our reduced value for candle power would be in error by no more than two per cent.

Table VI. shows that the mean deviation from the mean of observations corrected in this way is a little over two per cent., and that 14 out of 20 values were in error by 2.1 per cent. or less. In the case of corrections for rate, the mean deviation is over six per cent. while but one out of 14 values deviated by less than two per cent. and only four by less than four per cent.

In other words, by correcting for flame height an error of less than two per cent. may reasonably be expected, and the probability of making an error greater than four per cent. is small; while in correcting for rate, errors of eight per cent. and nine per cent. are of common occurrence.

The values corrected for rate might, perhaps, be more consistent if the rule were followed to reject all observations in which the rate fell below 114 or above 126 grains per hour. Similarly, the errors in the values corrected for flame height might be smaller if observations made at extreme flame heights were to be rejected. Indeed, it is one of the chief advantages of the method, that the observed flame height furnishes a criterion for the rejection of any observation which is regarded as doubtful. In this discussion, however, in order to be equally fair to both methods, no observations have been rejected.

The results of these photometer observations confirm fully those obtained by the use of the bolometer in determining the variations of light standards, and show very conclusively that the fundamental assumptions on which the bolometric tests were based, were entirely justifiable. If we compute from Table VI. the mean value of the flame height-intensity ratio, as determined by the bolometer, we find that it is just the same as the mean value from *all* the observations.

In section III. of this report it was shown that the English candle is subject to sudden variations in intensity which are sometimes as large as 15 per cent. Many of these sudden drops were noticed while making the photometer observations, and they all had the same characteristics as are shown by the bolometer

curves. Fig. 9 shows a group of these observations—group F in the tables. Assuming that the time intervals between the various photometer settings were equal, points were plotted showing the relation between intensity of the candle and time, and between flame height and time. It will be seen at once that the two curves are of very similar characters. The flame height gradually increased, and with it the intensity, until, when the height of 48 mm. had been reached, there was a sudden drop, the change in intensity amounting to 12 per cent.

These curves evidently show in an imperfect way variations precisely similar to those which are so faithfully reproduced by the galvanometer needle. In view of these qualitative and quantitative results, it would seem to be impossible to doubt the reliability of the bolometer as an instrument for making such tests.

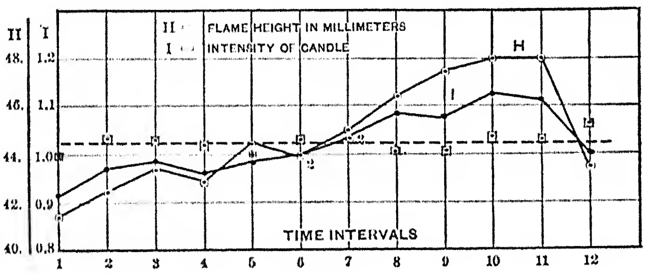


FIG. 9.

V.

THE GERMAN STANDARD CANDLE (VEREINSKERZE).

The specifications for the manufacture of this candle, adopted upon the recommendation of a committee of the German Association of Gas Manufacturers, are very careful and complete. The candle is made only under the immediate supervision of the association and is sold by them. The directions for its use are most minute. The photometric measurements are to be made only when the flame has reached its normal height, of 50 mm., the rate of consumption of paraffin being disregarded. The candle is made of the purest paraffin, has a uniform diameter of 20 mm. the number of strands of wick, also, being carefully specified. In the earliest recommendations for the use of this candle it was directed that the candle be allowed to burn freely, and when the flame height had reached 50 mm. then the photo-

metric settings were to be made. In later years, however, the recommendation was made that the wick of the candle be cleaned or snuffed in order to insure the reaching of the standard flame height more quickly. The use of this candle as an official standard has been abandoned even in Germany in favor of the Hefner lamp, and, consequently, it is not necessary to deal very extensively with tests which have been made of it.

Lummer and Brodhun[1] in determining the intensity of the Hefner light in terms of the German candle, using glow lamps as intermediate standards, investigated the performance of the candle with a good deal of care. Measuring the height of the flame by means of a cathotometer, they experienced difficulty in seeing the exact point of termination of the base of the flame, and also from the fact that the top would split up into three

TABLE VII.

Flame heights in millimeters.	Intensity.		
	I.	II.	III.
44	0.398		
45	.405		
47	.410	.389	0.380
48	.419	.398	.390
49	.426	.405	.396
50	.434	.410	.398

points, and when the height was near the normal height, the flame would smoke. Their measurements show that the ratio between flame height and intensity with this candle is not a fixed one.

At one time the height remained at 50 mm. for some minutes, and the following settings were made: 0.412, 0.420, 0.420, 0.424, 0.430. During this time the edges of the crater melted off to some degree.

Bolometer tests[2] of this standard were made by members of the present committee. They do not, however, show its behavior when used under normal conditions. They indicate a more uniform performance, except for continual small variations than is

1. Lummer and Brodhun: *Zeitschr. für Instrumentenkunde*, vol., 10, p. 119.
2. Sharp and Trumbull, *Phys. Rev.*, vol. ii, p. 1.

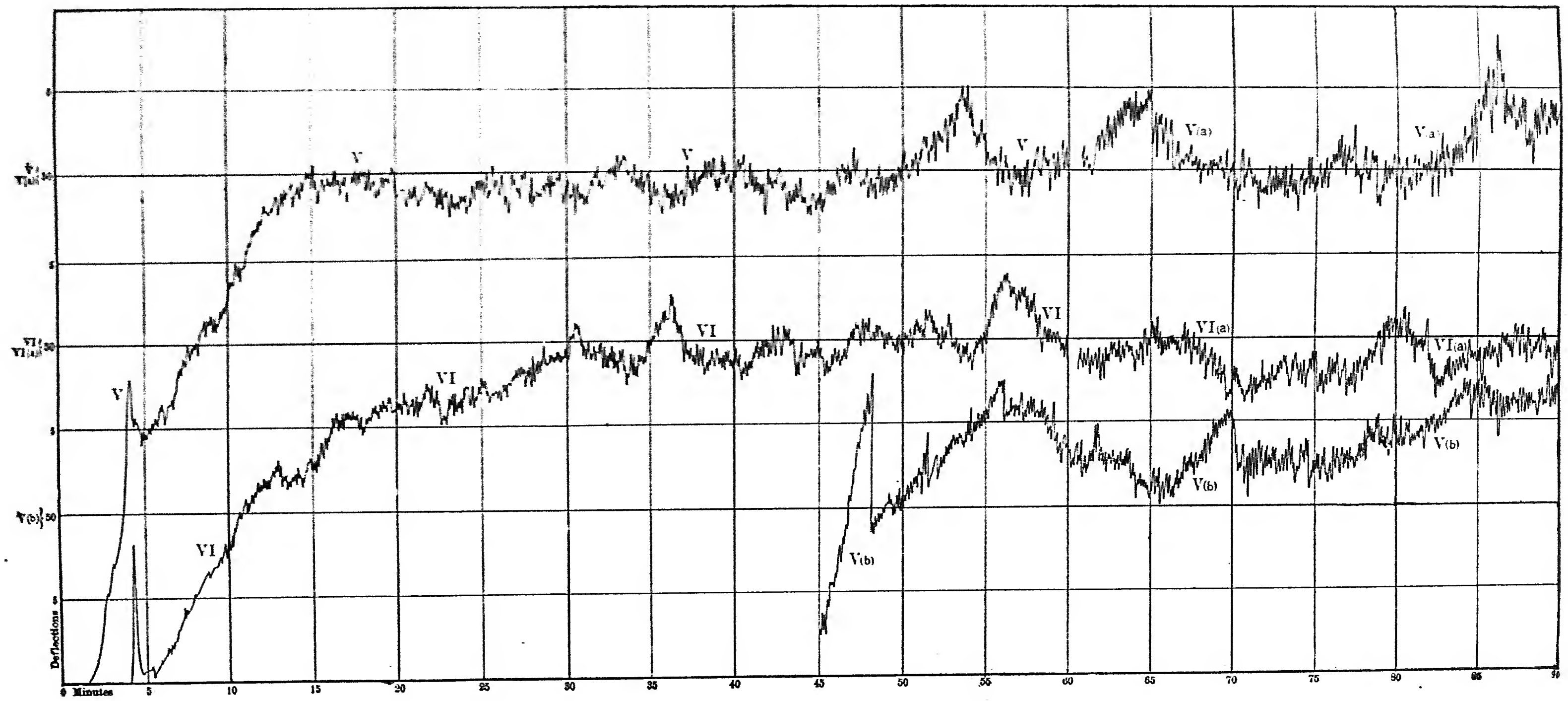


FIG. 10.—The German Candle.

seen in the case of the British candle, and show clearly the greater care which is used to secure uniformity of wick, etc.

The data from which the curves in Fig. 10 are obtained are analogous to those presented in tables I and II. It will not be necessary to give them here. The following general statements concerning the results embodied in Fig. 10 may, however, be deemed of interest.

Curves V. and V. (*a*) and VI. and VI. (*a*) were obtained from candles burned in exactly the same way as III. and III. (*a*) and IV. and IV. (*a*), the time interval between V. and V. (*a*) being one hour, and that between VI. and VI. (*a*) being 41 minutes. Curve V. (*b*) was taken with the remainder of the candle used for V., relighted on another night; so that it is entirely independent of V. and V. (*a*) excepting that the same candle was used.

When we come to compare the English with the German candle, we notice, first, that the variations of the English candle were much larger and that large variations were much more frequent. The German curves are free from the semi-periodic drops which characterize the English. Curve I. at 24 minutes reaches a value which is 1.255 of the mean ordinate of the English candle curves. Curve IV. at 49 minutes drops to a point which is only 0.77 of the mean ordinate. The total variation is, consequently, 46.5 per cent. Both of these curves were taken with the same candle, but on different nights. The highest point of the German curves is on V (*a*) at 86 minutes. The ordinate reaches a value which is 1.155 of the mean. The lowest point is on VI. (*a*) at 72 minutes, and is 0.915 of the mean. The total variation was consequently 24 per cent., or only about half that shown by the English candle.

Moreover, we see from the table that the percentage deviation of curve III., 55–60 minutes, from the mean of the English candles is + 10.45 per cent. Curve IV., 35–40 minutes, shows a deviation of — 13.08 per cent. The total deviation for a period of five minutes is 23.53 per cent for the English candles. In the German candles the maximum positive and negative deviations for five-minute periods are + 11.52 per cent and — 10.43 per cent. respectively. These give a total of 21.95 per cent. and exhibit a performance but little better than that shown by the English candle.

VI.

The Methven Screen.

Dibdin's results of tests of this standard are even less significant of what the standard will do from day to day under varying conditions, than are his tests of candles, because here he compared two gas flames with each other, and they must have been similarly affected by varying atmospheric conditions. He found in his observations, which are given in his report of 1885, for determinations made by one operator, a maximum fluctuation of 5 per cent., frequent fluctuations of from 2 per cent. to 3 per cent., while 37 per cent. of the measurements were within 1 per cent. of the mean of all. From observations by three different operators, the maximum was found to be 6.7; many of the fluctuations were of from 3 per cent. to 6 per cent., while 12.5 per cent. of the observations were within 1 per cent. of the mean. Using the carburetted gas, a result perhaps slightly better than the above was obtained, but with gas of varying purity he found large errors entering when the gas was bad. For example, a flame giving 18.50 candle power when compared with the Methven burning 16-candle gas, gave when 10-candle gas was used in the Methven 31.30 candle power, indicated in one test, and 22 candle power indicated in another. This shows clearly that the variation of candle power with the Methven is not a definite function of the richness of the gas, especially if the gas is poor. Dibdin's observations in this respect have been confirmed by the work of Rawson.[1] His conclusion is in contradiction to that of Heisch and Hartley, who found no variation whatever when the standard was supplied with gas running from 13.65 candle to 22.4 candle. Dibdin says: "These results . . . . must seriously militate against the adoption of the plain gas Methven slit standard, however useful it may be as a handy instrument in ordinary works. The arrangement for carburetting is, on the other hand, a very different thing and capable of very reliable work." In his other report, he says, "Observations with different operators cannot be considered as very satisfactory, for generally more experience with the carburetted gas is required than would appear necessary." In this second report, he found for the deviations, using carburetted gas, a maximum value of 5 per cent., while 74 per cent. of the observations were within 1 per cent. of the

1. Rawson: Electr., 17, p. 479, 1886.

mean. He found also that bad results were obtained if the burner and chimney had not had time to become thoroughly warm before use. By using several chimneys of varying thicknesses, he found no serious error resulting from this cause.

Rawson[1] found in investigating this standard, that it was difficult to determine just when the flame height was three inches, on account of the flickering of the flame. He found also changes in the rate of flow of gas when the eye could hardly detect any change in flame height. His observations show that the light emitted varies rapidly with changes in the quality of the gas when the gas is bad to start with. This is confirmed also by the work of the Dutch Commissioners, who found for the candle power of the Methven standard, using 15-candle gas, 2.05 candle power, while for 18-candle gas the candle power was 2.23.

Methven[2] has discussed the effects of changes in temperature and pressure, on this standard. The effect of increased pressure

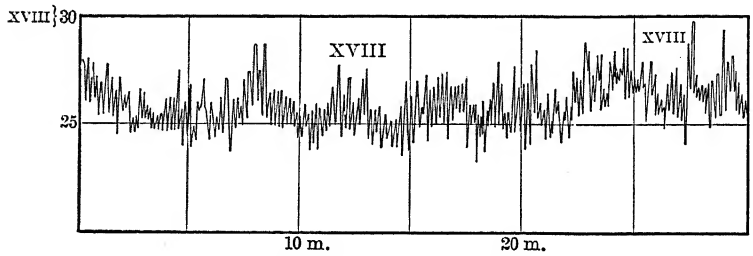


Fig. 11.—Methven Screen.

and temperature is not to increase the size of the flame on account of more rapid flow of gas, but in burning denser gas in a denser atmosphere the size is diminished. With lower pressure and higher temperature the density of the flame is less, and the draught of the chimney is less. Less oxygen flows, and the flame takes on a brownish color. If the specific gravity of the gas is low, the intensity is much increased; if high, the increase is less marked. The draught is determined by the difference between the temperature of the room and of the flame, and hence for equal quantities of gas, and varying room temperatures the intensity will be different. A given gas gave 15.93 c. p. at 3°, 9 *C;* and 16.90 c. p. at 22°, 2. This accords with the results of M. Bremond, who found for a given gas at different elevations

1. Rawson, loc. cit.
2. Methven: *Dingler's Polytechnishes Journal*, vol. 277, p. 276, 1890.

above the sea-level a decrease in intensity of 0.742 per cent. per 100 feet increase in elevation. Moreover, the intensity is 10 per cent. less when the flame burns in moist air than when the air is dry. Coal-gas burning in a Harcourt burner shows under similar circumstances a diminution of 13 per cent.

Fig. 11 and 12 exhibit specimens of the bolometric curves obtained by your committee in the case of the Methven screen. Curve XVIII. was taken with the Argand burner connected directly to the gas-pipes in the building. The pressure of the gas was controlled only by a large regulator on the main pipe leading into the building. The consumption of gas in the building was constant during the run. The curve shows many large, but quick variations and certain decided waves. The other Methven curve was taken with a gas-holder of about 10 gallons' capacity interposed between the gas main and the Argand burner. The effect of this in smoothing the curves is very marked, and indicates

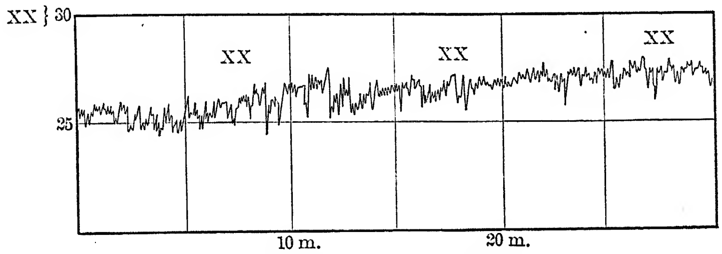


FIG. 12.—Methven Screen.

that for photometric purposes the Methven screen is much im proved by the interposition of a rather capacious reservoir. The use of such a reservoir tends to minimize the effect of changes of pressure and to absorb any waves in the gas due to water in the pipes or some similar cause.

The way in which these waves of variable pressure produce fluctuations in the amount of radiation, is by causing changes in the quality of those portions of the flame which cover the slit. At times the top of the flame becomes forked, so that not all of the slit is covered by it. It happens perhaps more frequently that some of the non-luminous portion of the flame rises so as partially to cover the slit. In either case the result is seen in a deviation of the curve. It must be true, also, that the amount of luminous radiation suffers a much larger proportional change than the total amount of radiation; hence the deviations recorded

on the curves are too small to represent correctly the fluctuations in luminous intensity. There is an additional reason for these fluctuations, which is to be found in the slight protection against draughts of air which the wide open chimney used on the London Argand burner affords. In this respect its action is in marked contrast with that of the smaller close chimney of the Carcel lamp, which, of course, can be used only with much richer gases than ordinary illuminating gas.

VII.

A. Vernon Harcourt's Pentane Standard.

This standard[1] consists of a gas flame produced in burning a mixture of 20 volumes air with 7 volumes of pentane vapor. The flame is $2\frac{5}{16}$ in. height, the orifice through which the gas passes being $\frac{1}{4}''$ in diameter. The rate of consumption of the gas is $\frac{1}{2}$ cu. ft. per hour. The standard temperature for making the mixture is 60° F., and the standard pressure is 30 in. of mercury. The pentane used is the most volatile portion of American petroleum, and is obtained by repeated distillation of gasoline, which has been purified by treating with sulphuric acid, and afterwards with a solution of sodium hydroxide. The distillation is carried on until the whole passes over at 120° F. The product is nearly pure pentane, $C_5 H_{12}$. It is mixed, however, with a small portion of hexane, which, it is claimed, will not affect the illuminating power of the gas, since it is so similar in chemical composition to pentane. The pentane evaporates more quickly than ether, and is nearly insoluble in water. Its specific gravity at 60° F. is between 0.628 and 0.631; its vapor is 2.5 times as heavy as air. The flame produced is said to be steady when the gas is rich and the flame not too large, while an additional advantage consists in the fact that the rate of consumption of gas at a given flame height furnishes a check upon the purity of the gas.

Dibdin, in his tests made in 1885, used an apparatus set up by Harcourt himself. He found that when the air gas was made over fresh water in the gas holder, his results did not agree exactly with those obtained when the water had been previously used for the same purpose. The size of the holder, and the exact

1. A. Vernon Harcourt, Report of the proceedings of the British Association, 1877. *Chemical News*, 36, 108. *Electrician*, 11, 188.

purity of the gas seemed to have no effect upon the candle power given. From his measurements, the maximum fluctuations were found to be 1.9 per cent., while 90 per cent. of the measurements lay within 1 per cent. of the mean. Five different operators, using the same burner, made measurements which were not so concordant. Their maximum deviation was 3.5 per cent., while only 44 per cent. fell within 1 per cent. of the mean. Dibdin found the steadiness of the flame to be affected but little by disturbances in the room, etc. This is quite opposed to the results of Heisch and Hartley, who had found that small disturbances in the neighborhood caused very considerable and very troublesome fluctuations in the size of the flame.

Dibdin's report of 1888 shows the maximum variation to have been over 5 per cent., and 80 per cent. within 1 per cent. of the mean. He says: "The facts brought out by the inquiry have shown that the method of preparing the air-gas is at once easy and safe; that the measurement of the volume of gas used is simple and reliable; that the adjustmeut of the flame height is a matter of certainty; its steadiness all that could be desired when due care is taken and proper apparatus employed; and that the quality of the light afforded is precisely the same as that of the standard comparison flame."

The committee of the British Association, in 1888, reported that the pentane standard was reliable and convenient, and fulfilled all the conditions required of a standard of light. They found that the light was not altered by using pentane of specific gravity of 0.628 or 0.632 instead of the normal specific gravity 0.630. Out of 117 tests which they made, only one showed a variation of one per cent., and there were no larger variations than that.

VIII.

The Pentane Lamp.

(Woodhouse and Rawson pattern.)

In the pentane lamp we have a flame of pentane vapor set free from a wick, and burning inside a metal chimney cut away in the middle. The height of the flame is adjusted by noticing when the point of it plays inside the limits of a vertical slit in the upper part of the chimney. The distance between the upper and lower parts of the chimney determines the intensity of the light. It can be adjusted to give one candle or one-and-a-half candles.

Dibdin, in his report in 1888, states it to be remarkably steady. The maximum fluctuation which he found was 2.5 per cent. while 97 per cent. of the tests fell within 1 per cent. of the mean.

The committee of the British Association in 1888 reported that out of 118 tests but two showed fluctuations of 1 per cent., which was a maximum.

Rawson[1] compared the pentane lamp with the glow lamp on a photometer bar. He found for the pentane lamp absolute steadiness for periods of from 15 to 75 minutes. Variations were due to two causes:

(1) The changes of temperature in the air of the room, which can be minimized by immersing the lamp-font up to the stopper in water.

(2) The escape of pentane vapor through the stopper. This may be prevented by placing a drop of glycerine on the stopper. Mr. C. H. Clifford of the Massachusetts Institute of Technology[2] compared two pentane lamps with each other and found a maximum deviation of 1.57 per cent., while five out of 18 observations varied by more than 1 per cent. from the mean. He says: "The Harcourt pentane lamp as a legal standard would seem to be a satisfactory one as far as mere intensity and constancy of illumination are concerned, although the uncertain quality of pentane and the care required in using the lamp are undoubtedly objectionable."

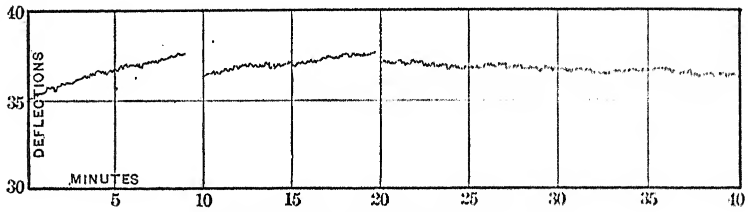


Fig. 13.—Harcourt Pentane Lamp.

But a single reliable curve Fig. 13 has been obtained by the bolometer for this standard. The great steadiness of the flame due to the chimney employed is manifest at once. This form of chimney evidently is quite as efficient in this respect as is the Carcel lamp chimney and it is not subject to the disadvantages of the latter form.

1. *Elec. World*, London, vol. xii., p. 251.
2. *Tech. Quarterly*, 1890.

The lamp was adjusted to give 1 C. P. and had been burning 1½ hours before the curve was begun. During the first nine minutes of the curve the height of the flame changed, remaining, however, always within the limits of the vertical slit. The intensity changed during these nine minutes by 6.7 per cent. of itself. The flame was then readjusted. During the next nine-and-a-half minutes a similar change took place, the total variation being 2.9 per cent. After a second readjustment the flame settled down to a fairly stable condition. The flame height was a trifle low at the end of the curve, at which time the intensity had fallen to a minimum value of 1.4 per cent. below the mean. At the end of the second ten minutes the intensity was 1.4 per cent above the mean. Omitting the values at the beginning of the curve, which may perhaps not be quite so reliable as the rest, the total variation is seen to be 2.8 per cent. The changes produced in the two readjustments were 3.2 per cent. and 1.4 per cent. respectively. Of course, from this single curve nothing can be said concerning the reproductibility of the standard. The bolometer shows the intensity of this lamp to be less than 1 C. P., and photometric measurements have confirmed this result.

Weighty objections to this lamp as a standard are the unstability of the flame under certain conditions, and the lack of a well-defined mark at which the flame height can be adjusted. It is to these faults that the variations during the first twenty minutes of the curve are due.

Liebenthal[1] investigated the relation between the height of the flame and intensity of the light, finding the following values: For the flame point at the bottom of the slit the intensity was 97.9; for the flame point one-third of the way up the slit, 99.5; one-half of the way, 100; two-thirds of the way, 99.5; at the top, 97.5.

Accordingly he adjusted it at the middle of the slit, taking that flame height as corresponding to normal intensity. He says: "Still greater variations of flame height produce changes which are not negligible, even for technical purposes. The intensity is about eight per cent. less than normal when the point of the flame projects by a small amount (about seven mm.) over the top of the slit, and a further decrease of at least seven per cent. is found when the flame becomes still larger. More than this, on account of the heating of the parts of the lamp, until a certain

1. Liebenthal, *Elektrotechnische Zeitschrift*, vol. xvi., p. 655.

thermal equilibrium is reached, which is about 30 min. after lighting, two things occur:—

(1). The flame increases continually so that the wick must be steadily lowered.

(2). The intensity increases and finally goes over into a constant value which is several per cent. larger than the original value. Only when such a stationary condition has been reached, and the flame height changes but gradually, can one leave the lamp unwatched for a little while without fearing that it may become overheated."

By making many photometric measurements, extending over a long time, using a glow-lamp as a secondary standard, and by determining each day the hygrometric state of the air, by means of an Assmann's hygrometer, Liebenthal investigated the effect upon the intensity of the light which the moisture in the air exerted. From 75 observations of this sort the intensity of the Harcourt lamp in terms of the Hefner light as unit can be expressed by the following equation:

$$y = 1.232 - 0.0068\,x,$$

or

$$y = 1.232\,(1 - 0.0055\,x),$$

where y represents the intensity, x the watery vapor in liters to each cubic metre of dry air, free from carbon dioxide. This formula holds good between the limits of observation which were from 4 to 18 liters, and gives us a variation of about 0.6 per cent. per liter of moisture. The mean deviation of observed intensities from those computed from the equation was found to be 0.81 per cent., and the maximum deviation was 1.6 per cent.

By making observations under varying pressure in a physician's pneumatic cabinet, the following relation between intensity and barometric height was obtained.

$$\Delta\, y = 0.00049\,(b - 760),$$

where b represents the barometric height in millimetres.

Hence, to a change in barometric hight of 40 mm. corresponds a change in intensity of two per cent.

The Dutch Light Standard Commission found the Woodhouse and Rawson pattern of the Harcourt lamp to be the most promising form of light standard. They objected to the use of pentane, as not being of definite chemical composition, and a modified form of lamp was arranged to burn a mixture of 100 parts of ethyl ether and nine parts benzol. Their lamp reproduces all

of the essential features of the Harcourt lamp. One important modification is that they make the chimney and upper portion of the wick tube detachable. This can be placed on a base by which illuminating gas can be admitted, lighted and burned until all these parts of the lamp have reached a stationary temperature. The chimney and wick tube are then replaced upon the font, and upon lighting, the lamp is ready to be used in photometric measurements. The materials which are burned in the lamp can readily be obtained in a pure state, and are not excessively expensive. They found that for slight changes in the amount of benzol added, when the proportions recommended were approximated to, the intensity of the lamp varied very slowly. Two series of ten settings each of two of these lamps against each other showed a variation of ± 0.52 per cent, while their intensities were equal. The maximum variation was ± 2.10 per cent.

IX.

Hefner Lamp.

Herr von Hefner-Alteneck[1] was led to the construction of the lamp which bears his name, as the result of his experiments upon simple benzine lamps. These lamps had round flames and large chimneys. With them he found excellent photometric results, using different grades of benzine as combustibles. He pointed out and insisted upon the necessity for using a combustible of known chemical composition. Accordingly an investigation was made of the intensity of the flames given by five fuels of different chemical composition, including among them commercial amylacetate. These showed only insignificant differences in intensity as long as the flames were adjusted to equal heights. He concluded from this that the height of the flame is the significant factor in defining intensities.

As a result of this experiment, he proposed the following lamp for giving a light of unit intensity. From a metal base projects a wick tube of German silver, 8 mm. in interior diameter, 0.15 mm. thick, and 25 mm. high. The flame height is adjusted at 40 mm. by the use of a "sight," or a Krüss's optical flame measure. The wick fills the tube without being compressed, and is arranged so that its height can be adjusted with nicety. Since the combustible, amylacetate, $C_7H_{14}O_2$, volatilizes at a low tem-

1. *Elektrotechnische Zeitschrift*, vol. iii., p. 445, and vol. v., p. 20.

perature, the wick does not need to be brought up to the top of the tube and does not char. The lamp should be burnt at least ten minutes before being used for photometric purposes.

The results of his investigation of the effect of impurities in the amylacetate are shown in the following table :[1]

TABLE VIII.

Amylacetate diluted with	Specific gravity.	Hourly consumption in grams.	Deviation from normal rate.	Deviation from normal intensity.
			Per cent.	Per cent.
20 per cent. fusel oil.......	0.8645	9.96	+ 6.9	− 2.0
2 per cent. diamylen.......	0.8725	9.24	− 0.8	0
5 per cent. alcohol and 4 per cent. castor oil..........	0.8745	9.88	+ 6.0	Impossible to measure.
10 per cent. isobutylacetate and 10 per cent. amyl-alcohol.....	0.869	9.28	− 0.4	+ 0.4
50 per cent. alcohol	0.8408	12.92	+ 30.	+ 40.
Pure	0.8735	9.318		

His conclusions were that variations in intensity due to the impurities which are most common are unnoticeable, while large variations from the normal are detectible through a change in the rate of consumption. Thus the rate furnishes a check both on the purity of the material and the intensity of the light.

Liebenthal[2] found that an increase or decrease in the diameter of the wick tube by 1 mm. caused a diminution of 1 per cent. in the intensity. Hence the given diameter corresponds to a maximum intensity. On the other hand, 1 mm. change in the height of the wick tube produced a corresponding change in intensity of only 0.2 per cent.

He investigated also the relation between flame height and the intensity of the light emitted. Measuring the flame heights by a cathetometer, he found the mean error to be 0.08 mm. to 0.09 mm. Using the ordinary "sight," the mean error was 0.5 mm., which may increase to 1 mm. if the eye is fatigued. This is due to the disturbing influences of the non-luminous mantle about the point of the flame. From this cause large errors may enter. With Krüss's optical flame measure, the mean error was 0.3 mm., which corresponds to an error of 0.9 per cent. in intensity.

1. von Hefner-Alteneck: *Elektrotechnische Zeitschrift*, vol. xxv., p. 323.
2. *Elektrotechnishe Zeitschrift*, viii, p. 504, and ix, p. 96, taken from *Journal für Gasbeleuchtung und Wasserversorgung.*

The mean variation of flame height during several periods of time was the subject of another part of his investigations. His results were as follows:—

Between the 12th and 23d of October, the mean variation of the flame height of a lamp, *A*, was 0.32 mm.

Between October 24th and 29th the mean variation of *A* was 0.19 mm., and of *B* 0.035 mm. Between October 30th and November 30th the mean variation was 0.16 mm. for both *A* and *B*.

Finally he found that the intensity of the Hefner light could be expressed, with a sufficient degree of accuracy, as a linear function of the flame height. For heights less than 40 mm.,

TABLE IX.

	Lamp H II. mean Intensity.	Deviation from mean.	Percentage Deviation.	Mean Error of each group.
			Per cent.	Per cent.
	0.3521	— 0.26	— 0.73	± 0.35
	58	+ 0.11	+ 0.31	.38
	53	+ 0.06	+ 0.17	.23
	30	— 0.17	— 0.48	.44
	53	+ 0.06	+ 0.17	.32
	56	+ 0.09	+ 0.25	.44
	62	+ 0.15	+ 0.42	.60
	26	— 0.21	— 0.59	.54
	63	+ 0.16	+ 0.45	.27
Means.....	0.3547		± 0.40	± 0.40

this is represented by the first of the following equations; for heights greater than 40 mm., by the second:—

$$i = 1 + 0.025\ (h - 40).$$
$$i = 1 - 0.034\ (40 - h).$$

The exact relation which he found is shown in Fig. 14.

Investigations for the purpose of determining the value of the illuminating power of the German standard candle, in terms of the Hefner light, Lummer and Brodhun[1] compared four Hefner lamps with a glow lamp. They obtained data in Table IX, concerning one of the lamps. These results show what degree of accuracy can be expected of this standard. From the intensity

1. *Zeitschrift für Instrumentenkunde*, vol ix, p. 119, 1890.

as given by them, the deviation of each set of measurements from the mean intensity has been computed, and this deviation has been reduced to percentages.

The intensities of the other lamps were as follows:

Lamp $H_I \begin{cases} 0.3545 \\ 0.3569 \end{cases}$ Lamp $H_{III} \begin{cases} 0.3539 \\ 0.3558 \end{cases}$ Lamp $H_{IV} \begin{cases} 0.3468 \\ 0.3455 \\ 0.3452 \end{cases}$

Mean intensity of H_I, H_{II} and H_{III} was 0.3547.

Mean error was $\pm$ 0.13 per cent.

The deviation of the intensity of H_{IV} from the mean of the other was 3 per cent. The inner diameter of the wick-tube of H_{IV} was 8.22 mm., and hence the deviation of its intensity from the mean of the others confirms Liebenthal's observation as noted above.

The conclusions which were reached as a result of the official tests of the Hefner lamp at the Reichsanstalt, are as follows:[1]

"Investigations have shown on the whole a favorable result. The intensity is not dependent upon dimensions of the lamp in such measure as to demand too much of the maker; the thickness of the walls of the wick tube is of importance, since if they are too thick the intensity is lessened, while if they are too thin the flame no longer burns steadily. A degree of purity of the combustible which the chemist cannot easily reach is unneces sary, while impurities can be detected by simple tests. Less favorable is the dependence of the intensity upon the surrounding air, the presence of carbon dioxide having a strong influence. For the setting of the height of the flame either the "sight" or the optical measure may be used; whichever is used, it should be adjustable."

Liebenthal[2] investigated the variations in intensity of the Hefner light as dependent upon hygrometric state, pressure, and amount of carbon dioxide in the atmosphere. The method in which this investigation was carried out has already been indicated in our discussion of the Harcourt lamp. He took a series of observations extending through 12 consecutive months, of the intensity of the Hefner light as compared with a glow lamp, measuring each day the hygrometric state.

The glow lamp had previously been used for a long series of tests of many Hefner lamps and so its intensity in terms of that

1. *Zeitschrift für Instrumentenkunde*, vol. xiii., p. 257.
2. *Elektrotechnische Zeitschrift*, vol. xvi., p. 655.

unit was well known. From the table which is given, it can be seen "that the Hefner lamp in March, April and May, also in Oct. and Nov. had an intensity almost equal to 1, while during the months of June to Sept. its intensity was on the average 2 per cent. smaller, and in the months of Dec. Jan. and Feb. it was larger by about the same amount. To be sure, the variations in the single months are not insignificant. For the month of May for example, the variation was in the neighborhood of five per cent. The least intensity, 0.948, occurred in July, and the greatest, 1.033; in Jan. and Feb. moreover the variation of the light intensity during the year under consideration amounted to 8.5 per cent."

"Further, it is to be noticed that during this year the mean deviation of the intensity from that obtained from the previous calibration of the glow lamp was $\pm$ 1.78 per cent."

"The graphical representation as well as the computation by the method of least squares shows that the intensity, y, of the Hefner lamp can be represented with great accuracy as a linear function of the moisture of the air, x. We have, between the investigated values of hygrometric state of from three to 18 liters,

$$y = 1.049 - 0.0055\, x,$$

or

$$y = 1.049\,(1 - 0.0053\, x).$$

"The intensity decreases uniformly with increasing moisture by an amount equal to 0.55 per cent. per liter. The difference between the observed variations in intensity and those computed from the equation, is at its maximum 0.9 per cent. while its mean value is $\pm$ 0.41 per cent. In the original definition of the light unit the moisture of the air was not taken into consideration. Since the deviations which follow from this are in the mean $\pm$ 1.78 per cent. the original definition of the Hefner light is sufficient for almost all technical purposes. If one wishes a greater accuracy, some datum concerning the hygrometric state of the air must be inserted in the definition of the light unit. It would be necessary then to determine for what proportion of moisture the intensity of the Hefner lamp should be taken equal to 1. On practical grounds, it is advisable to select for this a mean value; accordingly that hygrometric state was taken as normal, which made the above measurements give a value for the intensity of the glow lamp which was in exact accord with the previous extensive calibration of the same, namely, 8.8 liters

per cu. m. of dry air. The light unit designated by the Reichsanstalt in its official tests as the Hefner light is accordingly, taken exactly, the intensity of the Hefner lamp when the hygrometric state of the air is such that there are 8.8 liters of moisture per cu. m. of dry air."

The influence of pressure changes between the limits of 735mm and 775mm. is shown to be a small one. If we call $\triangle\, y$ the change in intensity corresponding to the barometric height b, we have:

$$\triangle\ y\ 0.0032 - 0.00011\ (b - 730)\ \text{or}$$
$$\triangle\ y = 0.00011\ (b - 760).$$

This represents a variation of 0.1 per cent. per centimetre.

"To determine the influence of carbon dioxide, four series of observations have been carried out in the following way. Into the well-ventilated photometer room carbon dioxide was admitted, from a cylinder of the gas, and simultaneously with the photometric measurements, the proportion of carbon dioxide, was found by Hempel's method, the moisture also being determined. If we represent the intensity by y' and the proportion of carbon dioxide in liters per cu. metre of dry air by x', the following formula holds between 0.6 liter to 13.7 liters carbon dioxide — $y' = 1.022 - 0.0072\ x'$, in which the first constant on the right side of the equation refers to the intensity corresponding to the mean hygrometric state. Accordingly a variation in the amount of carbon dioxide by one liter, causes a variation of the intensity by 0.0072 Hefner light, that is by about 0.7 per cent."

"From a comparison of the formulæ for moisture and carbon dioxide we see that equal volumes of watery vapor and carbon dioxide lower the intensity in unequal ratios, which are to each other as 1 : 1.30. Therefore volume for volume, the carbon dioxide influences the intensity to a greater degree than the watery vapor. Yet in reality the influence of the carbon dioxide is slight compared with the influence of the moisture on account of its smaller quantity. In the freshly ventilated photometer room of the Reichsanstalt the proportion of carbon dioxide in the air varied between 0.62 and 0.93 liters; to this variation corresponds a change in intensity of 0.2 per cent. which is quite within the limits of the errors of observation."

"We see therefore, that in a sufficiently large, well-ventilated photometer room, the carbon dioxide does not exercise a damaging effect. Very small rooms, especially all enclosed photometric apparatus may give rise to considerable errors, since the air in

them is very soon vitiated to a marked degree by the addition of watery vapor and carbon dioxide, and by the removal of oxygen."

By other than German photometrists the Hefner light has been shown rather scant favor, the chief objection to it being that the flame is too red in color. Dibdin indeed, made a rather extensive test of it, and found excellent results as far as steadiness goes, 90 per cent. of the tests being within one per cent. of the mean, and this in spite of the fact that he took the perhaps unwarrantable liberty of increasing the flame height to 51 mm., to make the intensity of the lamp equal to that of the British candle. The committee of the British Association found only four among 118 tests, which deviated more than two per cent. from the mean, and 11 which deviated more than one per cent. from the mean.

The Dutch Commission, to which allusion has already been made, dismisses the Hefner light from consideration because of

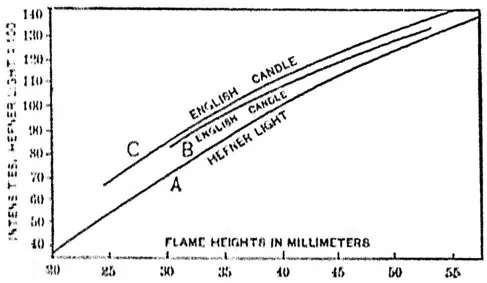


FIG. 14.

the instability of the flame. They found the following values for deviation in intensity. For a lamp using the optical flame measure, the mean deviation was ± 0.71 per cent. and the maximum deviation ± 2.83 per cent.: for a lamp using a sight the corresponding values were ± 1.08 per cent. and ± 4.32 per cent.

The bolometric investigation made by your committee upon the Hefner lamp is entirely corroborative of the previous testimony concerning the accuracy and steadiness of that standard. Typical results are given in Fig. 15.

Curve VII. of this figure was taken when a window in the farthest corner of the room was raised about 2 cm. Curve VII. (*a*) was begun 25 minutes after the end of VII., the lamp having burned during the interval, and the flame having been readjusted in height. The window was closed. The marked difference between the two curves is due to the stoppage of this slight draft.

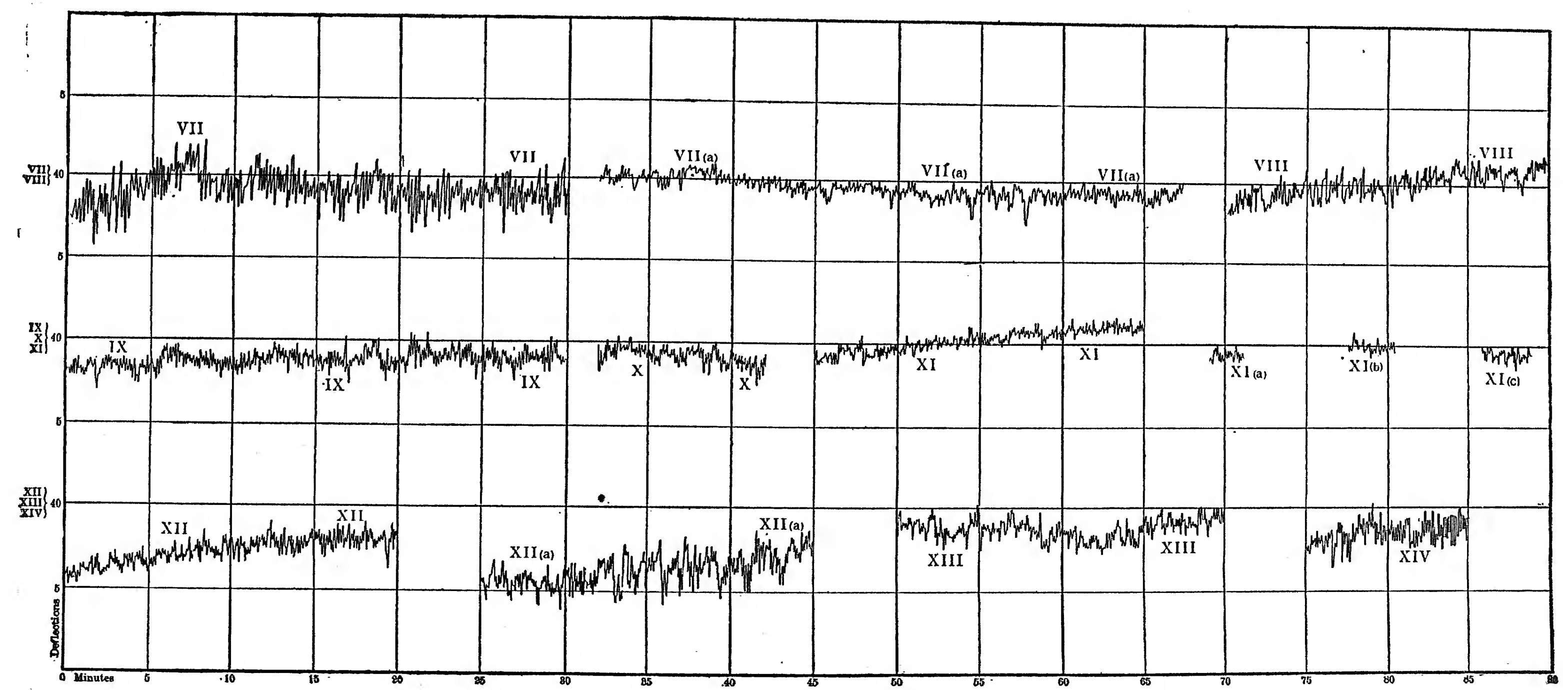

VII
VII
VII(a)
VII(a)
VII(a)
VIII
VIII
IX
IX
IX
X
X
XI
XI
XI(a)
XI(b)
XI(c)
XII
XII
XII(a)
XII(a)
XIII
XIII
XIV
Deflections
Minutes

FIG. 15

In taking curve X., the sensitiveness was adjusted at 32.5, or about double the standard sensitiveness for the other tests of the candles and of the Hefner lamp. Then a computation was made, from the law of inverse squares, of the distance at which it would be necessary to place the lamp in order to produce the ordinary deflection. The fact that the ordinates of this curve agree with those of the curves taken with a sensitiveness of 16.2 shows that the method of getting the sensitiveness by taking only first throws of the galvanometer needle, was an allowable one.

The length of time that the lamp had been burning, when curves VIII., XI., XII., and XIV. were begun, was 20 minutes or less. Each of these curves shows a gradual increase in the amount of radiation. This emphasizes the advisability of lighting the Hefner lamp a considerable time before it is to be used in photometric work.

It is to be understood that, in the case of each of these runs, except the one when curve VIII. was taken, the greatest care was exercised to have the room as quiet as possible. Curve VII. was the first of the Hefner lamp curves taken, and the need of extraordinary care in excluding drafts of air was not appreciated.

Data concerning the curves in Fig. 15 are given in the following Table X.

TABLE X.

	Time: Minutes on curve.	Mean ordinates and their mean.	Deviations from the mean ordinate of the curve.	Deviations reduced to percentages.	Deviations from mean ordinate of all the Hefner lamp curves.	Deviations from general mean reduced to percentages.
CURVE VII.	0-5	38.81	−0.50	−1.27	+0.17	+0.44
	5-10	40.02	+0.71	+1.81	+1.38	+3.57
	10-15	39.64	+0.33	+0.84	+1.00	+2.59
	15-20	39.19	−0.12	−0.31	+0.55	+1.42
	20-25	39.03	−0.28	−0.71	+0.39	+1.01
	25-30	39.19	−0.12	−0.31	+0.55	+1.42
		39.31				
CURVE VII. (a).	30-35	39.96	+0.44	+1.11	+1.32	+3.42
	35-40	40.27	+0.75	+1.90	+1.63	+4.22
	40-45	39.83	+0.31	+0.79	+1.19	+3.08
	45-50	39.54	+0.02	+0.05	+0.90	+2.33
	50-55	39.09	−0.43	−1.09	+0.45	+1.16
	55-60	39.03	−0.49	−1.24	+0.39	+1.01
	60-65	39.19	−0.33	−0.84	+0.55	+1.42
	65-70	39.27	−0.25	−0.63	+0.63	+1.63
		39.52				

	Time: Minutes on curve.	Mean ordinates and their mean.	Deviations from the mean ordinate of the curve.	Deviations reduced to percentages.	Deviations from mean ordinate of all the Hefner lamp curves.	Deviations from general mean reduced to percentages.
Curve VIII.	70–75	38.91	−1.12	−2.80	+0.27	+0.70
	75–80	39.83	−0.20	−0.50	+1.19	+3.08
	80–85	40.54	+0.51	+1.28	+1.90	+4.92
	85–90	40.86	+0.83	+2.08	+2.22	+5.74
		40.03				
Curve IX.	0–5	38.26	−0.54	−1.39	−0.38	−0.98
	5–10	38 62	−0.18	−0.46	−0.02	−0.05
	10–15	38.97	+0.17	+0.44	+0.33	+0.85
	15–20	38.87	+0.07	+0.18	+0.23	+0.59
	20–25	39.04	+0.24	+0.62	+0.40	+1.03
	25–30	39.03	+0.23	+0.59	+0.39	+1.01
		38.80				
Curve X.	30–35	38.89	0	0	+0.25	+0.65
	35–40	39.16	+0.27	+0.70	+0.52	+1.34
	40–45	38.62	−0.27	−0.70	−0.02	−0.05
		38.89				
Curve XI.	45–50	39.42	−0 83	−2.06	+0.78	+2.02
	50–55	40.50	−0.10	−0.25	+1.51	+3 91
	55–60	40.50	+0.25	+0.62	+1.86	+4.81
	60–65	40.92	+0.67	+1.67	+2.28	+5.90
		40.25				
Curve XI. (*a*,) (*b*) and (*c*.)	70	39.37	−0.24	−0.61	+0.73	+1.89
	75–80	40.00	+0.39	+0.98	+1.36	+3.52
	85–90	39.46	−0.15	−0.38	+0.82	+2.12
		39.61				
Curve XII.	0–5	36.47	−0.90	−2.27	−2.17	−5.62
	5–10	37.21	−0.16	−0 43	−1.43	−3.70
	10–15	37.85	+0.48	+1.28	−0.79	−2.05
	15–20	37 94	+0.57	+1.52	−0.70	−1.91
		37.37				
Curve XII. (*a*)	25–30	35.53	+0.67	−1.85	−3.11	−8 03
	30–35	35.73	+0.47	−1.30	−2.91	−7.52
	35–40	36.38	−0.18	+0.50	−2.26	−5.84
	40–45	37.18	−0.98	+2.71	−1.46	−3.78
		36.20				
Curve XIII.	50–55	38.84	+0.15	+0.39	+0.20	+0.52
	55–60	38.74	+0.05	+0.13	+0.10	+0.26
	60–65	38.20	−0.49	−1.27	−0 44	−1.14
	65–70	39.00	+0.31	+0.80	+0.36	+0.94
		38.69				
Curve XIV.	75–80	38.46	−0.16	−0.41	−0.18	−0.47
	80–85	38.78	+0.16	+0.41	+0.14	+0 36
		38.62				

Mean ordinate of first five minutes of Hefner lamp curves = 38.645.
Reduced to true deflections = 38 66.

The highest point on the Hefner curves is 1.093 of the mean, and the lowest is 0.867, giving a total deviation of 22.6 per cent. This large deviation is not so significant, however, as a similar one in the case of candles, since in dealing with the Hefner lamp we have to do with an adjustable flame, and these fluctuations may indicate only that a slight readjustment was needed.

The most important question in regard to the Hefner lamp is the accuracy with which it can be adjusted to its normal light intensity. Some idea may be formed of this by noting in Table II. the percentage deviations of the first five minutes of each of the curves from the mean ordinate of the first five minutes of all the curves. The maximum deviation will be seen to be 8 per cent., while the mean deviation is 2.3 per cent.

Curves XI., XI. (*a*), XI. (*b*), XI. (*c*) show the results of successive attempts to adjust the height of flame to just 40 mm., the wick having been lowered between each one. No zero and sensitiveness readings were taken between the beginning of XI. and the end of XI. (*c*).

They are of special value to show the accuracy with which the flame can be adjusted under given unchanging conditions.

From the numerical data upon which these curves are based, the following table of mean ordinates has been compiled.

TABLE X (*a*).

Curve.	Mean ordinate.	Deviation from mean.	Percentage deviation.
XI. (5m)	39.42	— 0 14	— 0.35
XI. (*a*)	39 37	— 0.19	— 0.48
XI. (*b*)	40.00	+ 0.44	+ 1.11
XI. (*c*)	39 46	— 0.10	— 0.25
Mean 39.56		Mean....... ....0.57	

If, now, the assumption be made that the light-giving efficiencies of the English and German candles and of the Hefner lamp are equal, it is possible to get their relative intensities by a comparison of the mean ordinates of their curves. Reducing the deflections as read on the telescope scale to angular measure and taking double the tangents of these angles, we have for the true deflections the following values:—

$$\text{British candle} = 41.06$$
$$\text{German candle} = 50.40$$
$$\text{Hefner light} = 38.66$$

From these values we get the following ratios:

$$\frac{\text{German candle}}{\text{British candle}} = 1.2275$$

$$\frac{\text{Hefner light}}{\text{British candle}} = 0.9415$$

Various determinations of $\frac{\text{Hefner light}}{\text{British candle}}$ are given in Table X (*b*).

TABLE X (b).

OBSERVER.	HEFNER. British Candle.
[1] Sharp. Candles reduced for rate	0.872
[1] Sharp. Candles reduced for flame height	0.892
[2] Sharp & Turnbull. Integration of energy curves	0.941
Violle	0.98
[3] Reichsanstalt investigations, mean value	0.876
[4] Netherland Photometry Commission	0.921
[5] S. Schiele. Mean value	0 881

The value 0.94 rests on the assumption that the radiant efficiencies of the candle and Hefner flames are equal. Since the Hefner flame is distinctly redder in color than the candle flame, its radiant efficiency is probably smaller, and consequently the value 0.94 is too large. A difference in the radiant efficiencies of the two sources of less than 0.2% would serve to bring this value down to 0.88. The preponderance of evidence in favor of the value 0.88 is very great. It is probably true that the very

1. See Part IV of this report. The intensity of a Hefner lamp was determined by comparing it with the glow-lamp used throughout this investigation, the candle power of which was, subsequently, accurately known from many measurements.

2. Vide supra.

3. This is the mean of a long series of determinations made by different observers at different times using candles from various sources. The measurements were taken at normal flame height of 45 mm. See Beglaubigung der Hefner Lampe. Zeitschrift für Instrumentenkunde 13 p. 257.

4. An abstract in German by Krüss will be found in the Journal für Gasbeleuchtung und Wasserversorgung. 1894.

5. Schiele. Report of committee on the comparison of the Hefner lamp and German and British candles. Journal für Gasbeleuchtung und Wasserversorgung. Band 32. 1889. p. 757. Also Dingler's Polytechnisches Journal, 274. p. 540. The measurements were made at normal flame height of 45 mm.

best way we have at the present time of determining candle power is to use the Hefner lamp and then to reduce by the use of this ratio.

X.

Platinum Standards.

(1) *The Violle Standard.*[1]—Violle conducted a long series of experiments upon this radiation of silver and platinum raised to various temperatures, and investigated also the light emitted by these metals when, after being melted, they have just reached the point of solidification. For platinum he obtained by the use of the thermo-pile, a curve of cooling.

This shows that molten platinum cools very rapidly at first, but as it nears its point of solidification, its rate of cooling becomes much slower and just upon solidification, there is a sudden rise in the intensity of radiation. After this, the cooling goes on rapidly once more. Violle's proposition for a light standard is this: He would define the unit of light as the light radiated by one square centimetre of platinum at its point of solidification. He melted the platinum by the use of the oxy-hydrogen blow pipe. A hollow cylinder through which cold water is circulating and which is pierced by an aperture one square cm. in size, is then passed above the molten metal. The light is reflected along the photometer bar, and its changes of intensity are followed by means of the photometer. The setting which is made at the time of the final increase in radiation (éclair) is the one which is taken as representing the intensity of the standard. From Violle's measurements, a very good result was obtained for this standard. Unfortunately, however, he used the Carcel lamp as a comparison standard in much of his work, and the deviation of his various measurements of the intensity of the platinum standard may be due entirely to his comparison standard.

But few physicists have investigated this unit, the reason probably being that it is so expensive to install and difficult to operate. At the Reichsanstalt,[2] investigations have been made by Lummer and Kurlbaum, in which the platinum was melted by Violle's method, and also by the use of an electric current. Their definitive results have not been published, but it has seemed advisable to them to abandon the use of the Violle standard as impracticable.

1. Violle, *Annales de Chem. et de Phys.*, Ser. 6, vol. iii., p. 373.
2. Lummer and Kurlbaum, *Elektrotech. Zeitschrift*, vol. xv, 1894, p. 474.

Many attempts have been made to substitute for the large mass of platinum which Violle used, a thin strip, and to take measurements upon the platinum at its melting point rather than at its point of solidification. None of these has led to very favorable results. The best one probably is that proposed by Siemens. This has been carefully investigated at the Reichsanstalt. Hundreds of meltings were made, the greatest precautions being always employed, but the deviations were found to be as large as 10 per cent. or more. The conclusion reached from this by Lummer and Kurlbaum is that thin platinum foil, when heated electrically, often tears apart long before the whole of the radiating surface has reached the melting point.

Mr. C. R. Cross[1] investigated the radiation from platinum wires of various diameters when brought to the melting point by the electric current, and came to the conclusion that the fusing point depended upon the previous history of the platinum, the gas occluded, etc.

Draper, Schwenler and others have proposed as a light standard, the light emitted from the surface of a platinum foil of definite dimensions when a given amount of electrical energy is being expended in it. None of these suggestions have led to any practical result.

(2) *The Lummer and Kurlbaum Platinum Unit.*—From their previous investigation of the Violle and Siemens standards, Lummer and Kurlbaum[2] concluded that absolutely pure platinum should be used as a radiating surface. They found that even a rough surface of the same became white when incandescent, and that moreover the platinum tended to purify itself when repeatedly heated and cooled. On the other hand, it would seem that the fixed temperature could be neither the melting point nor the solidifying point of the platinum, so that a method of defining the temperature of an incandescent platinum surface was sought for. These observers appear to have reached the end sought, even though the light unit obtained has not entirely fulfilled their wishes in respect to simplicity. "It demands," they say, "a complicated arrangement of delicate apparatus, and great experimental skill." They define the unit of light as that emitted by one square centimetre of incandescent platinum when the

1. *La Lumière Electrique*, 22, p. 507.

2. Lummer and Kurlbaum: *Elektrotechnische Zeitschrift*, vol. xv. (1894), p. 474.

ratio of the portion of its radiation transmitted by a specially designed water cell to its total radiation is equal to $\frac{1}{10}$. The platinum foil is stretched beneath a suitable covering arranged to make uniform the currents of air, and is heated by an electric current from storage batteries. The current required is from 50 to 80 amperes. By the introduction of resistance, the current is adjusted to such an intensity that the radiant efficiency becomes such as desired. In front of the platinum foil is a tube pierced with an aperture one square centimetre in size, and cooled by water circulation. The water cell is made of a cylindrical glass ring, closed by two parallel quartz plates. The quartz plates are each one mm. thick, and the thickness of the layer of water is 2 cm. The bolometer used, is that constructed by Lummer and Kurlbaum for such work, and consists of very thin platinum foil, which has been cut away in the form of a grid.

The standard which carries the platinum foil, is made to rotate through 90°, so as to face either the bolometer or the photometer. Comparisons of the intensity of the light were made with a glow lamp by means of the Lummer–Brodhun photometer. In determining the constancy of the radiation of the glowing platinum the bolometer was used. Throws of the galvanometer were taken at intervals of one minute, the result showing an admirable degree of constancy. In determining the radiant efficiency, the throws of the galvanometer showing the total radiation, and radiation through the cell were taken with the bolometer at such distances from the platinum foil that the galvanometer throws would be equal in the two cases; that is, the distances were in the ratio of $1 : \sqrt{10}$. In this way any errors due to the peculiarities of the galvanometer used were eliminated. It was found that an error of one per cent. in determining the radiant efficiency corresponded to an error of three per cent. in the intensity of light. Investigation of the radiation from chemically pure, and from commercial platinum foil showed a difference between them. At a high temperature the impurities of the commercial foil evaporate, so that the surface does not remain perfectly smooth, while the surface of the chemically pure foil becomes mirror-like. Reproductions of the standard could be made with chemically pure foil within one per cent.; with the commercial foil on the other hand, deviations of from two to three per cent. occurred. The aperture in the diaphragm could be measured within 0.01 mm. which corresponds to 0.2 per cent. change in light intensity. The cosine

law of radiation was found to be followed with such accuracy that no error resulted when the foil was not exactly parallel to the diaphragm. An error of 0.1 mm. in the thickness of the quartz plates of the water cell produced an error in the light intensity of 0.1 per cent., while an error of 0.1 mm. in the thickness of the layer of water produced an error of one per cent.

The greatest difficulty arose from the selective absorption of the material with which the bolometer strips were blackened. It was found to be impossible to get consistent results for the radiant efficiency with various bolometers covered with lamp-black, on account of the lack of uniformity in the thickness of the lamp-black covering on the different bolometers. A good result was obtained, however, by coating the bolometer strips with an electrolytic deposit of platinum black. A solution of definite composition was made, the bolometer strips and platinum strips were immersed in it, and a certain definite current was passed through for a definite time.[1] This resulted in a coating which was absolutely uniform, and has enabled consistent results to be obtained for the radiant efficiency. The conclusion reached as the result of these experiments is, that by using such apparatus and by taking careful account of all possible sources of error, a light unit is obtained which can be reproduced within one per cent.

XI.

The Blondel Arc Standard.

An isolated portion of the crater of the positive carbon of the arc has been suggested both as a primary and secondary standard. The suggestion came from Swinburne and S. P. Thompson independently, in 1892, and in a paper[2] read at the Electrical Congress at Chicago, the latter urges its adoption as a substitute for the Violle platinum standard.

That the light emitted from the crater is constant in quality and brightness was pointed out as early as 1878, by Capt. Abney. Recent measurements by Violle indicate a constant intrinsic brightness quite independent of the power expended in the arc.

1. For a bolometer of 8 cm.^{2} area, the solution is as follows: Platinum chloride, 1 part; water, 30 parts; Lead acetate enough to make one part to 4,000 parts water. The temperature of the solution should be 20°; the E. M. F. 4 volts; the current 0.25 ampere. The current is allowed to pass for 2 minutes. The bolometer strip is placed between two platinum electrodes so that both sides are equally blackened. See *Zeitschrift für Instrumentenkunde*, Aug. 1895.

2. "Proc. of the International Elec. Congress," p. 267.

His experiments were conducted between the wide limits of 10 and 400 amperes and corresponding power values of 500 and 34,-000 watts. Throughout this range Violle found the spectro-photometer to show no variation in quality of light emitted—a result which was corroborated by a photographic method. These results lead Violle to announce absolute constancy in the temperature of the crater surface—a constancy which he believes to be due to a true ebullition of the carbon at this point. This view is in accordance with those advanced by S. P. Thompson.

The temperature of the positive carbon Violle has determined to be 3,500°. This value, obtained by a calorimetrical method, must be considered as only approximate, owing to lack of knowledge of the specific heat of carbon at this extreme temperature.

To Blondel[1] is due the first attempt to put this standard into a working form. In the form of lamp adopted by him, an obliquity of 40° to 60° from the vertical is given to the crater surface by inclining the carbons about 20° from the vertical. The extent of the incandescent surface exposed is regulated by a diaphragm pierced by holes of different sizes, any one of which may be brought behind an opening in the screen by a simple rotation. The screen is cooled by a stream of water as in the Violle standard. For arc light photometry an opening of one square millimetre is recommended. The illumination on a screen 4 m. distant is then about 10 candle-metres. The best results are obtained by maintaining an arc of 5 mms. with a current density of 0.2 ampere per square mm. It is, of course, essential that the operator satisfy himself that he is getting light from the region of maximum brightness before making a photometer setting. This is ensured by momentarily dropping a small lens in front of the hole and projecting an image of the carbon points upon a screen placed at right angles to the photometer bar. Blondel has determined the intrinsic brightness of this source to be 158 candles—the extreme figures in his measurements being 150 and 163.

With reference to this standard, a question of paramount importance is that of the influence of the quality of the carbons upon the intensity. Since the impurities are volatilized at a relatively low temperature, a surface of pure graphite is left as the light-emitting coating of the crater, and it might be supposed that moderate variations in the quality of the carbons would be

1. Blondel, "Proc. International Elec. Congress," p. 323.

without effect on the intensity of the light emitted. Blondel's researches in this direction indicate the contrary. For cored carbons he obtains values as low as 130 candles for the intrinsic brightness. Even with homogeneous carbons there is a considerable variation—so much, indeed, that the utility of the device for anything but a secondary standard for arc light photometry is exceedingly doubtful.

Bolometric curves of an improvised arc standard consisting of a Schückert focussing arc lamp with oblique carbons, in front of

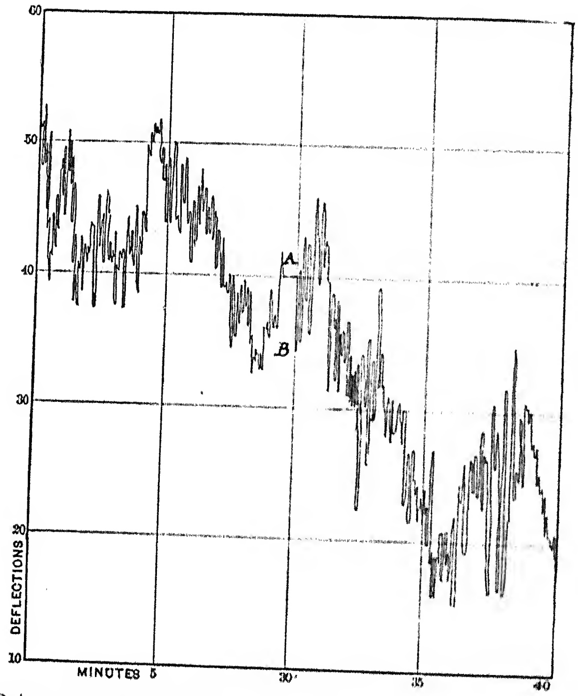


Fig. 16.—Between the points A and B there was a time interval of 20.5 minutes.

which a diaphragm was mounted, are shown in Fig. 16. It would not be fair, perhaps, to claim that in this experiment the conditions proposed by Blondel were successfully fulfilled. It may be said, however, that a reasonable attempt was made to do so on the part of practiced experimenters. The range of the sudden fluctuations which the curve exhibits may be taken as indicative of the difficulties which those are likely to encounter who use such a standard.

XII.

Kerosene Oil Lamps.

These lamps have been quite extensively used as secondary standards, and have been usually found very satisfactory.

Heim,[1] in studying the efficiency of various sources of light, used a kerosene lamp as a secondary standard, with good results.

A screened kerosene student's lamp has been found to give good results when used in the Edgerton photometer.[2] The screen was pierced with a round hole 12 mm. in diameter. With different kinds of kerosene, there was a slight difference in intensity. A series of ten observations, each with flame height varying from 65 mm. to 300 mm., showed a maximum variation in intensity of only 5 per cent. Changes in flame height amounting to from 12 to 25 mm. made inappreciable differences in intensity. The chimney used had a considerable influence on the intensity.

Herr von Hefner-Alteneck[3] has emphasized the points of excellence of kerosene lamps, claiming that they are superior to the Carcel lamp.

[4]In some of the measurements of the intensity of a candle as a function of the height of its flame, a kerosene lamp with an Argand burner, similar to the ordinary student lamp burner, was used. The upper part of the chimney was covered by a tightly-fitting cylinder of ferrotype iron, in such a way that the top of the flame was entirely hidden. This furnished a very steady source of light, after it had been burned long enough for the parts to become thoroughly warmed, and its intensity was unaffected by slight adjustments of the height of the flame, provided only that the flame was always high enough. Its intensity varied enough from day to day, however, to make its indications unreliable without a daily calibration.

A convenient form of kerosene lamp requiring no chimney, in many ways suited to photometric purposes, is the Hitchcock mechanical draught lamp, made at Watertown, N. Y.

1. Heim, *Lumière Électrique*, 26, p. 220.
2. *Polytechnic Review*, 1878, 5, p. 161.
Dingler's Polytechnisches Journal, 229, p. 48.
3. Von Hefner-Alteneck. *Elektrotech. Zeitschrift*, 4, p. 445.
4. Sharp. *Phys. Rev.* Part IV of this Report.

XIII.

The Carcel and Keats Lamps.

The Carcel lamp has been in use for very many years in France, and in the hands of experienced operators it is capable of yielding good results. The combustible is colza oil, the normal rate being 42 grams per hour. The most exhaustive tests of its behavior are those of Audoin and Bérard,[1] made under the directions of Dumas and Regnault. In these experiments the effect of the height of the wick, its nature and the height of the contraction in the chimney were studied with reference to the consumption of oil. The results indicate that it is unjustifiable to take the intensity as proportional to the rate of burning unless these variables are very carefully looked after, that is to say, unless the lamp is always used under conditions which it is very difficult to maintain constant.

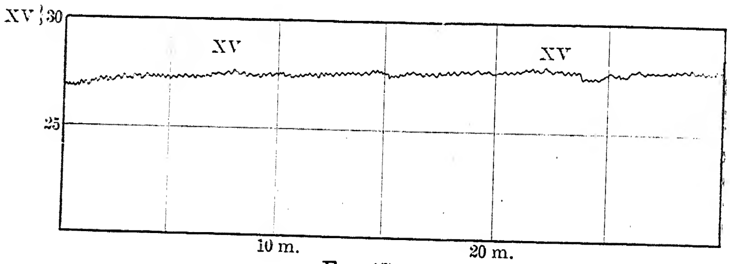


Fig. 17.

In countries other than France, the Carcel lamp has met with little favor, the results of tests showing considerable variations in behavior. Dibdin's results (1888) show a mean deviation from 33 sets of observations of 1.34 per cent., disregarding signs. The maximum was 4.1 per cent., 32 per cent. of the tests being within one per cent. of mean.

The Keats lamp shows a better behavior. Thus Dibdin found 71 per cent. of his observations within one per cent. of the mean, and a maximum variation of 3.4 per cent. On the strength of these comparative results he rejects the Carcel lamp in favor of the Keats. His report of 1888 shows, for the Keats lamp, a maximum deviation from mean of 9.4 per cent., 39 per cent. of the observations being within one per cent. of mean. He says: "The Keats lamp has been tried most thoroughly, but has failed in

1. *Annales de Chemie et da Physique*, 3d. series, vol. lxv.

practice to realize all that was formerly hoped for it, not so much from any inherent defect, as from the severe trial it makes, as compared with other systems, upon the patience of the observer."

Figure 17 contains a curve which is typical of the behavior of this source of light. It will be noted that the rapid fluctuations, characteristic of naked flames are almost absent.

The curves obtained in the study of the Carcel lamp all show clearly the great gain in steadiness of the flame resulting from the use of a proper chimney. The deviations were, for the most part, no larger than the swings of the galvanometer when the bolometer was unexposed to radiation. The variations extending over a considerable period of time are, however, by no means inconsequential. In the course of one test there was a total deviation in the mean ordinates of the five minute periods of 5.1 per cent., taking place in ten minutes. Another experiment shows a deviation of less than 0.8 per cent. in 35 minutes, and this is perhaps a more typical illustration of what may be expected from this lamp when burned under the best conditions. The highest and lowest points on any of the Carcel curves are 29.9 and 25.4 respectively, corresponding to deviations from the mean of the curves of + 5.7 per cent. and — 12.5 per cent., or a total of 18.2 per cent. The maximum deviations, for periods of five minutes, are + 5.3 per cent. and — 8.4, giving a total of 13.7 per cent.

XIV.

The Acetylene Flame.

A preliminary report has already been made to the Institute on the use of the acetylene flame as a light standard.[1]

In France, Violle has given some attention to this standard, and has published a note concerning it.[2] The latter recommends the use of a flat flame, maintained at a pressure of 30 cm. of water, the gas being mixed with air drawn in at the base of the burner. The entire flame may be used as the light source, or it may be screened to any desired amount. Under former conditions a consumption of gas of 58 litres per hour is reported as furnishing a light twenty times that of illuminating gas in an or-

1. Fessenden, Transactions, p. 507, June–July, 1895

2. *Comptes Rendus*, Jan. 13, 1896.

dinary burner. The distribution of luminous intensity in the spectrum of acetylene is found to coincide quite closely with that of platinum at the melting point.

XV.

Incandescent Oxides.

Concerning the sources of light which depend upon the incandescence of a glowing metallic oxide, a sufficient amount of

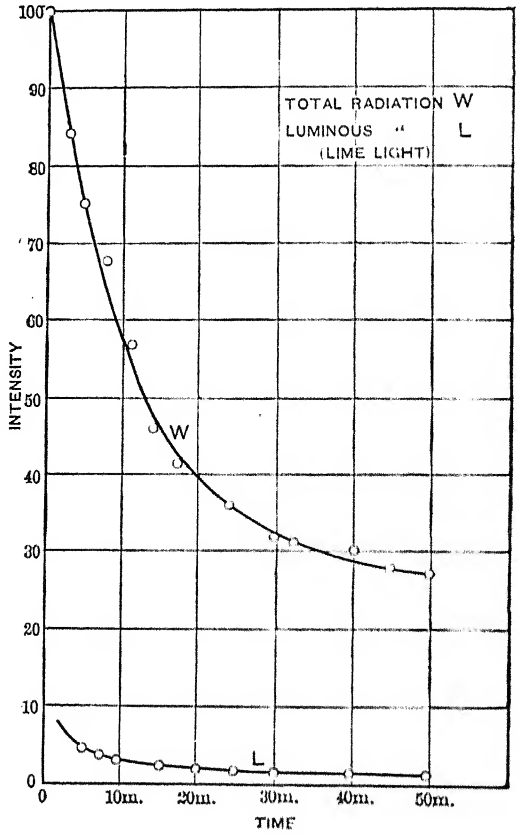


Fig. 18.

work has been done to show that, as a class, they possess a property fatal to their use as light standards. This property may be illustrated by means of measurements made by a member of the Committee on Light Standards.[1]

1. Nichols and Crehore, *Phy. Rev.*, vol. i, p. 161.

In these measurements, the radiation from the glowing oxide of calcium, namely, from a cylinder of the Drummond light brought to incandescence by the action of the oxy-hydrogen flame was tested by means of the thermo-pile and galvanometer. The intensity of radiation, as determined by means of the deflection, was measured as function of the time from the instant of ignition, for a period of fifty minutes. Similar curves were taken for the radiation capable of passing through water cells, which were interposed between the thermo-pile and the cylinder.

From the results, shown graphically in Fig. 18, there is seen to be a rapid and continual falling off in intensity, which, in the case of the total radiation, amounts to 60 per cent. during the first

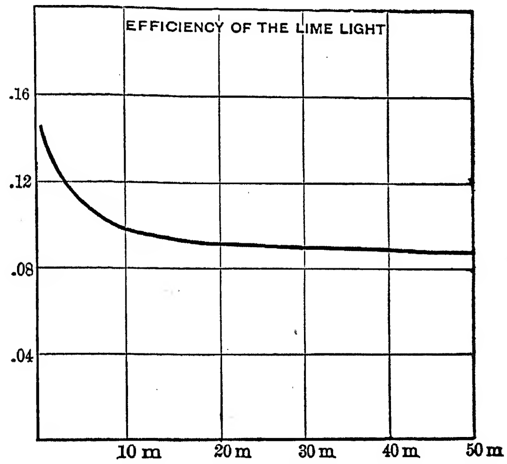


FIG. 19.

twenty minutes after ignition. This continues, but with decreasing rapidity, throughout the remainder of the time. At the end of fifty minutes, the intensity of radiation in terms of the initial intensity is about 27 per cent. By plotting the ratio of the curve of total radiation to that of luminous radiation, we obtain the radiant efficiency of the lime light as a function of the time. Were this curve a horizontal line, the conclusion would be that the quality of this source remained a constant, although the amount of light which it sends forth diminished. The curve which is given in Fig. 19 indicates, however, that the initial efficiency, which amounts to about 15 per cent., falls off in the course of fifty minutes to less than 9 per cent. The former cor-

responds to the radiant efficiency of the magnesium light, a value which is in accordance with the character of the light emitted by the freshly ignited lime. The latter shows a degree of incandescence inferior to that of the ordinary arc light. This change is in complete corroboration of the results of earlier photometric tests of this source.[1]

The measurements were supplemented by a photometric study of the lime light in the course of which the decadence of three

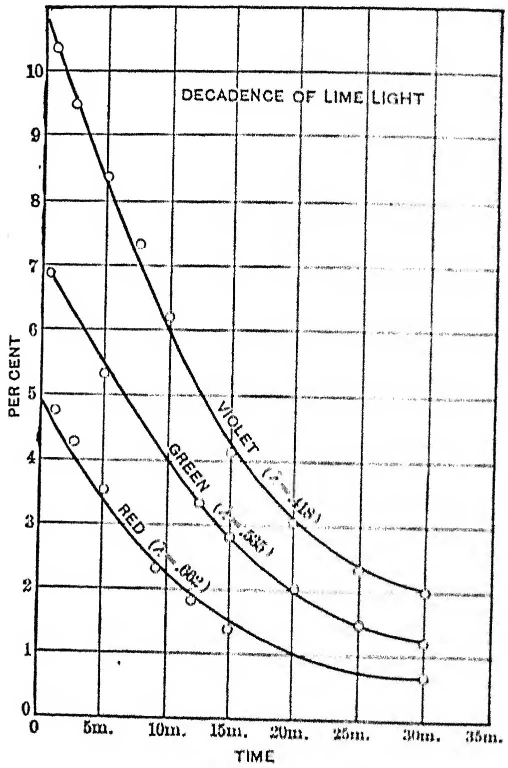


FIG. 20.

typical wave lengths was computed as a function of the time. These were, violet wave length 418, green wave length 535, and red wave length 662. Measurements of these regions of the spectrum during 30 minutes from the time of ignition, gave a curve the character of which is indicated in Fig. 20. It will be seen that in all three cases there is a very rapid diminution of

1. See Pickering, "American Academy of Arts and Sciences," vol. xv; also Nichols and Franklin, *American Journal of Science*, vol. 38, p. 103.

light. The falling off of the value amounts to more than 80 per cent. during 30 minutes. From these values, and from a complete study of the spectrum of lime under the conditions which it reaches after 30 minutes, it is possible to plot curves showing the relative distribution of light in lime light and gas light at different times after ignition. In Fig. 21 a set of such curves are given, which show graphically the manner in which the lime light falls off in intensity and varies in quality during the first half hour of its incandescence. These curves are very similar to those which

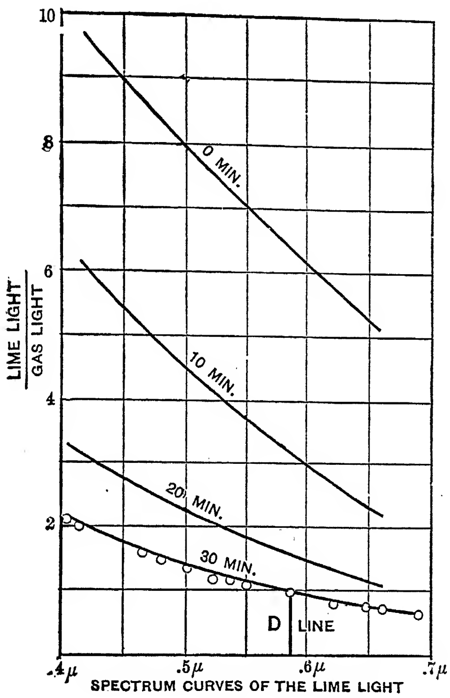


FIG. 21.

have been obtained with oxide of zinc heated electrically to a temperature of 1,000°[1].

Cursory observations, made in cases where disks of magnesium, oxide and zircon had been substituted for the lime, showed analogous behavior, although there seemed to be reason to believe that the decadence was not so rapid as in the case of lime.

1. Franklin, *Amer. Journal of Science*, vol. xxxviii., pp. 103.
See Nichols and Snow, *Philosophical Magazine*, (5) vol. xxxii., pp. 401.

Measurements made with the Welsbach incandescent mantle burners afford abundant evidence of the same tendency on the part of this source of light. Records of candle power and gas supply have been made covering several hundred hours[1].

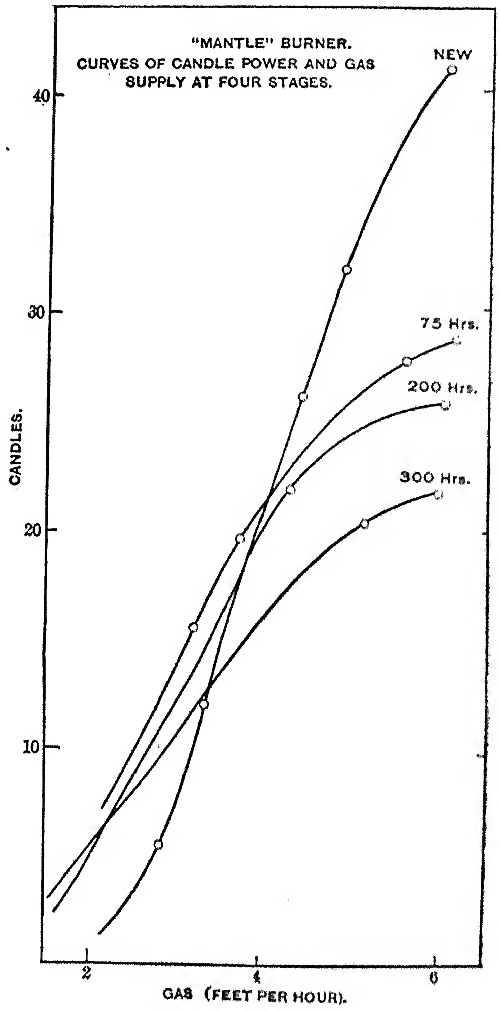


FIG. 22.

Fig. 22 which is constructed from the measurements of Ida M. Hill[2] shows the relationship between the candle power and gas,

1. See thesis of E. V. Stebbins, Mss. in the Library of Cornell University, also Nichols *Laboratory Manual of Physics and Applied Electricity*, vol. ii., p. 339.
2. Ida M. Hill, thesis in the Library of Cornell University, 1890.

supplied to a mantle burner of the form in use in 1890, when new, at the end of 75, 200 and 300 hours. Fig. 23 shows the curve of decadence with age of a later type of mantle burner, from the measurements of Mr. Stebbins. It is interesting to note the resemblance between the latter curves and the like curves of the incandescent lamp. It has recently been pointed out as a result of microscopical examinations of this burner, that there is permanent cause of the falling off in the candle power, namely a diminution in the radiating surface[1].

It should be said in this connection, that the rate of decadence of such burners after about 100 hours becomes very slow. It is possible that mantle burners properly aged by previous incandes-

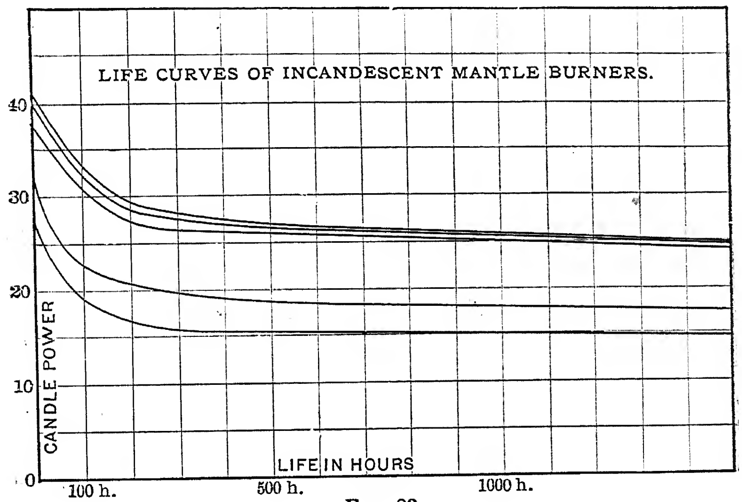


Fig 23.

cence might afford satisfactory standards. This committee has not as yet had the opportunity to test the properties of such a standard.

These data make it evident that if the light from an incandes-

1. St. John found that the net work of the mantle when new showed interstices nearly filled with the oxide, whereas in 100 hours the material had largely disappeared reducing the net work to a skeleton. St. John estimated the diminution in radiating surface at about 50 per cent. which would very nearly account for the falling off in candle power. (See "Wiedemann's Annalen," vol. 56, p. 433.) Photometric measurements of this source show, however, a greater relative loss of violet than of red light, and measurements of the radiant efficiency show that this likewise falls off. These two facts always accompany one another, and they indicate a change in the radiating power of the material.

cent metallic oxide is to be used in the production of a primary light standard, the method of composition must be one which continually introduces a freshly formed film. The light of a burning magnesium ribbon fulfills this condition, but there is as yet no practicable method of controlling the combustion of such a ribbon.

XVI.

The Glow Lamp as a Standard of Light.

It was at one time thought by some, that under carefully prescribed conditions of manufacture, glow lamps would give a sufficiently constant performance to make them of use as primary light standards. This suggestion, brought forth during the early days of glow lamp manufacture, has been shown to be of little value. During recent years, this source has been the subject of exhaustive tests on both sides of the Atlantic, with the result that its behavior is now well known.[1]

As a secondary standard, the glow lamp occupies an important place. Ease and constancy of regulation, and the possibility of securing a standard of nearly the same intensity and color as the light under test, are among its advantages. In fact, the glow lamp may be said to constitute the best secondary source at the command of the photometrist to-day, and it is accordingly largely used by lamp manufacturers in rating their product. There are, however, certain precautions to be observed in its use. Life tests of the older types of lamps were characterized by a marked fall in candle power during the early part of their life, after which the change became much more gradual. Recent tests by Ayrton and Medley[2] show, in the Edison-Swan lamps, an initial rise in the candle power. This rise, amounting in some cases to thirty-three per cent. during the first 124 hours of life, is not accompanied by a proportionate increase in energy consumed; that is, the efficiency rises during the early life of the lamp. The spark test shows in these lamps an improved vacuum after they have been burning for some time. That the

1. See, among others: Peirce, Transactions, vol. vi, p. 293; Nichols, N. Y. Electric Club Pamphlets, No. 27 (1890), and Thomas, Martin and Hassler. Transactions vol. ix, p. 271.

2. Tests of Glow Lamps, *Phil. Mag.*, May, 1895.

change in vacuum is the cause of the increasing light intensity does not seem to be established by experiment, since the improvement in the vacuum is found, to a marked degree, in lamps which show but slight rise in candle power.

But whether the early life of a lamp be marked by rise or fall in candle power, it is in almost all cases the period of most rapid variation, and hence it is important that this stage should be passed in lamps used as light standards. It is also important that the lamp should not be strained by an excessive voltage, as its later life will be accompanied by rapid decline in candle power. As soon as an incandescent lamp has been standardized, one or more copies of it should be made and put aside, in order that the original lamp may be examined at any time for change in intensity.

The steadiness of the energy supply is, of course, of prime importance. It is difficult to obtain satisfactory results with any source other than storage batteries. Moreover, the electrical measurements to which the intensity is referred must be made with accuracy. Roughly speaking, the candle power of a glow lamp is proportional to the cube of the watts expended in it, and to secure a constancy within 1 per cent. in the luminous source, the electrical energy supplies must be controlled within 0.4½ per cent. at least.

How very accurate a secondary standard the glow lamp becomes when properly handled, may be seen by a report of the Reichsanstalt in Charlottenburg.

It refers to an admirable investigation of glow lamps as secondary standards, made by Lummer and Brodhun[1], at the Reichsanstalt. They compared with each other two 65-volt lamps run at a pressure of a little less than 55 volts, determining by means of one, the change in intensity of the other when the latter was operated many hours.

In order that photometric measurements might be significant to within 0.1 per cent., measurements of current and potential were made accurate to within 0.01 per cent. using Clark cells and resistances of zero temperatures coefficient. Results of the comparison of two lamps, L and R, are given in Table XI.

1. Lummer and Brodhun, *Zeitschrift für Instrumentenkunde*, 10, 119, 1890.

The position of the glow lamp as the most reliable of secondary standards may be regarded as established. He who seeks to use it as a primary standard, will, however, have to face a number

TABLE XI.

Hours burned. R	4	Ratios of Intensities. L/R
1	1	0.8779
20	2	0.8764
62	3	0.8741
154	8.5	0.8724
211	13.5	0.8677

of difficulties. One of the chief of these consists in the fact pointed out some years since by Evans[1], that treated and untreated carbons possess very different properties. Weber[2] in his paper on the general theory of the glow lamp has determined the

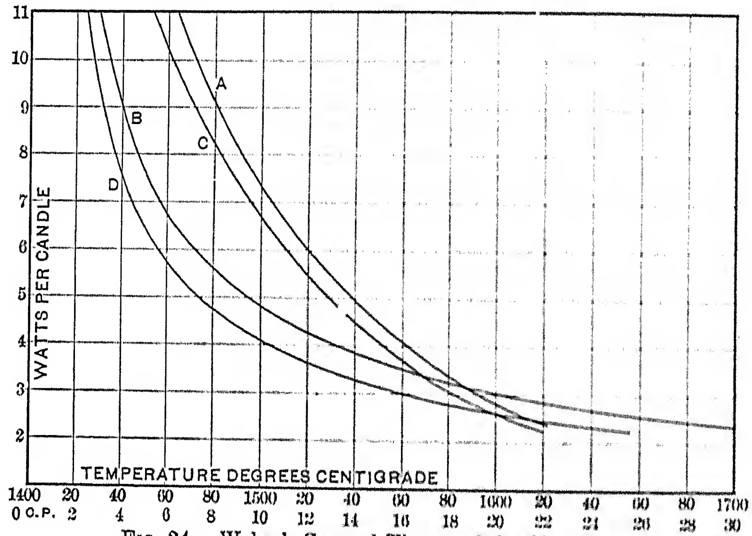


FIG. 24.—Weber's General Theory of the Glow Lamp.

Lamp of the Allgemeine Elektricitäts Gesellschaft.
A, Watts per Candle and Temperature.
B, " " " C. P.
Sunbeam Lamp.
C, Watts per Candle and Temperature.
D, " " " C. P.

emissivity of the two, and has shown that they are to be regarded as distinct varieties. That the difference between the

1. Evans, "Proceedings of the Royal Society," 1886.
2. Weber, *Physical Review*, vol. ii., p. 112.

black and the gray surfaced carbon is not negligible will be evident upon inspection of the curves in Fig. 24. These curves, which have been derived from data given by Weber, give the relations between efficiency and temperature, and efficiency and candle power respectively in the case of lamps with treated and with untreated filaments.

One of the members of your committee[1] has shown further

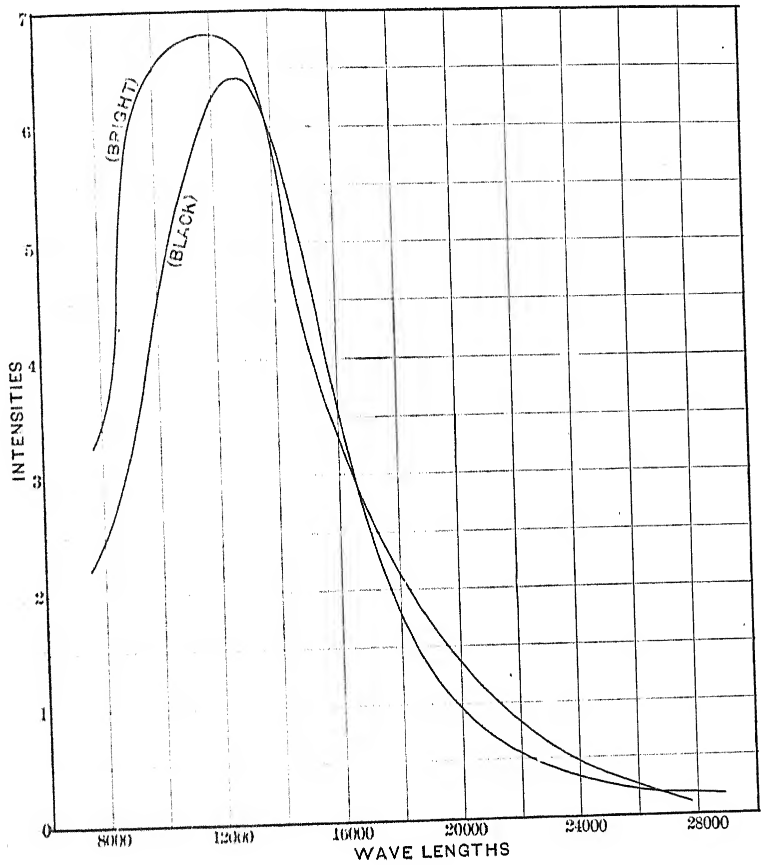


Fig. 25.

that the energy curves of the spectra of lamps with bright and with black filaments are far from being identical.

In Fig. 25 two such energy curves are presented. It will be seen that the curve of the lamp with bright filament has its maximum at a shorter wave length, and that the distribution of energy differs greatly in the two cases.

1. Nichols, *Physical Review*, vol. ii., p 260

Conclusion.

It is evident from the foregoing summary of previous photometric researches and from the report of the work of this committee, that of all standards thus far used, candles are the least reliable. It is also evident from the bolometric curves that naked flames are subject to sudden and rapidly recurring fluctuations that may be almost entirely eliminated by the use of a properly constructed chimney.

It seems likely that many of the difficulties which are unavoidable with flame standards may be overcome by the adoption of a standard consisting of some surface electrically heated to a standard temperature.

The definition of the degree of incandescence of such a surface appears at the present almost insuperable, but the committee is at work upon a method for the measurement of the temperature of incandescent carbon, which may lead to results looking towards a solution of the problem.

It also has in progress experiments looking to the production of a light standard in which not only the burning material but also the atmosphere shall be of known and definite chemical composition. Liebenthal's experiments indicate clearly that this is a necessary condition to the production of any invariable flame. In the preliminary experiments now under way, a flame of a mixture of two parts acetylene to one part hydrogen, burns in a current of pure oxygen, all the gases being dry. The flame produced by these means is of dazzling brilliancy, its color being comparable to that of the lime light. No accurate measurements of its steadiness or reproducibility have yet been made.

This experiment will include a spectro-photometric study as well as an investigation of the range of fluctuation to which it is subject under different conditions of combustion.

Physical Laboratory of Cornell University, May 1, 1896.

Discussion.

The President:—Gentlemen, this is an exceedingly interesting subject, and a very difficult one to deal with. In the last week or so the Institute has had requests from several outside sources to attempt to fix a standard of light. It is a matter which the Committee has taken up in the most exhaustive way, and any light that the members may be able to throw upon the subject in the way of discussion will be very welcome.

Mr. J. W. Howell:—I have some knowledge of the pressure which is being brought to bear upon the Institute urging them to establish a standard of light. I think it is indeed one of the most important subjects that has been brought before the Institute in a long time. I fully appreciate the absolute lack of a commercial standard of light, and I know that because scientific men have been unable to refer light to an absolute standard, they have been very loth to establish any arbitrary standard. That has left us in a condition of chaos, you might say, in regard to light standards. I think Dr. Nichols' paper has shown the condition of affairs very well. The fact that there are half-a-dozen different sources of light accepted by people as standards indicates how badly we need *one* standard. Now, what we need is—not an absolute standard of light, but something which is reproducible. Of course, we want to get the best thing, and that is the thing which is most easily reproducible. I think the Institute should continue the work of this Committee until they are well enough satisfied with their labors to issue a commercial standard; to state what is in their opinion the best available standard of light to-day, with the fullest possible description of what it is, where it can be obtained, and just how to use it. If that is issued under the authority of the Institute, I think it will undoubtedly become the commercial standard of the United States. In my experience with incandescent lamp work, the absence of a standard has been very severely felt, and the standards used by different people are so various that there is no agreement between them at all. I agree with Dr. Nichols that the amyl acetate lamp is the best that is known to-day, but it has not been generally adopted in this country. The amount of light obtained from it is so very small that that is a considerable objection. Regarding secondary standards, we always used an incandescent lamp. I think, under proper conditions they are very good secondary standards, and will retain their value for a considerable time. I will be glad to furnish Dr. Nichols with all the assistance I can in this respect, for we have used the incandescent lamp as a secondary standard for a number of years, and I shall be glad to give him the benefit of my experience.

Dr. Nichols:—The only form of amyl acetate lamp which the Committee has tried, is the standard form furnished the makers in Germany. The great difficulty which seems to exist with that lamp is the redness of the flame. The flame is, I might say, of a ruddy color, and there is a very considerable color difference between it and gas even, and still more between it and a high efficiency incandescent lamp. It has been shown by the very painstaking investigations of certain German students of this subject, that the atmospheric conditions, unless artificially controlled, are sufficient to produce a definite fluctuation in those standards, which consist of a naked flame. For example, the amyl acetate lamp has been tested at frequent intervals for two

or three years, and it has been shown to vary with the time of year, giving more light in certain months than in others. These curves of fluctuation vary with the meteorological conditions, especially with the amount of moisture in the atmosphere. The variations are such that it seems as though we must abandon the open air as a source of supply for flames and introduce instead of it an artificial supply. That is the idea which the Committee had in mind in the lamp which I have outlined on the blackboard, in which the hydro-carbon should be entirely enveloped in an artificial atmosphere of constant proportions, and thus be as far as possible free from the influence of meteorological changes.

MR. CARL HERING:—I would like to ask Dr. Nichols whether he has tried an amyl acetate lamp with a flat flame in combination with a Methven screen. It seems to me that such a combination might meet some of the objections to the use of that lamp.

MR. C. P. STEINMETZ:—I have been very much interested in the work done by the Committee with the acetylene flame as standard, since from my experience with acetylene I believe, that, if developed, the acetylene lamp would give an ideal standard of light, due to the ease of producing acetylene chemically pure, and the white color of the light, which is similar to that of the incandescent and the arc light, and thereby more convenient than the reddish amyl acetate flame. I have experimented a little with acetylene as an illuminant, and derived the best results with small flames, by having the gas issue at a low pressure, of 1 or 2 centimetres water column, through the hole of an ordinary blow-pipe. This gave a flame of something like 4 to 5 centimetres hight, of intense whiteness, of probably something like 25 candle power intensity.

There is one source of contamination, however, to be feared in the acetylene when derived directly from calcium carbide, which while immaterial for the use of acetylene as ordinary illuminant, would be objectionable for a standard of light. The acetylene is liable to contain a small percentage of hydrogen, which even in small quantities noticeably reduces the intensity of light. Therefore for the use in a standard lamp, I think it desirable to produce the acetylene gas from liquid acetylene, which is necessarily free from hydrogen.

[The paper by Mr. D. McFarlan Moore, presented April 22d, was then taken up for discussion; see page 85.]

[COMMUNICATION BY PROF. R. A. FESSENDEN RECEIVED AFTER ADJOURNMENT.]

I do not think any one can read this report without feeling an admiration for the manner in which the writers have done the work they set out to do, and an indebtedness for the knowledge they have given us.

It is apparent that a standard must have the following properties,' amongst others, if it is to be satisfactory.

It must not use a wick.

It must not be much affected by draughts.

It must not be much affected by moisture or by change of barometer.

It must burn a definite chemical compound.

In January, 1895, I notified the Chairman of the Committee, Prof. Houston, that I was working upon an acetylene standard, and in April I made a report of progress which was printed in the June TRANSACTIONS.[1] I regret to say that on account of pecuniary difficulties the University has been unable to contribute the amount promised, and work has hence been carried on but slowly. At that time, as is mentioned in the report, it was found that the most suitable pressure was 12 inches, and that the flat flame gave the best results. It is a source of gratification to know that M. Violle, as a result of his experiments, (*L'Elect.* p. 76, 1896,) has also found a pressure of 30 cms. and a flat flame to be most suitable, so that this point may be considered as decided.

My further work on the subject has been devoted to ascertaining the best means of producing the flat flame, since the object aimed at is the production of a standard which can be duplicated in any instrument shop by any physicist in the same manner as a Clark cell. The most obvious plan is that of using a narrow slot and adjusting the edges at a certain distance apart, the distance being measured by interference methods. On working out the plan in detail, however, it was found that the expansion of the materials used would be apt to introduce errors which could not be neglected. Moreover the method would be difficult and expensive. After some amount of experimenting, the method shown in the drawing;[2] in which two jets of gas from two circular orifices of the same diameter impinge upon each other at an angle of 90°, thus forming, as experiment shows a very evenly luminous flame, was finally decided upon, for the following reasons.

It is easily shown, by very simple reasoning, that if we have to make an orifice of a certain definite area, that of all shapes, the circular one is that in which a given amount of error will make the least amount of percentage error. Thus, with a slot one inch

1. TRANSACTIONS, vol. xii., p. 500.
2. In the Figure, A and C form one toroidal chamber, surrounding B.

long and $\frac{1}{100}$ inch width, an error of $\frac{1}{1000}$ inch would make ten per cent. error in the area, while if the orifice were circular and of equal cross-section, the same absolute error would make less than two per cent. of difference.

Moreover the circle is of all forms that most easily made accurately. With a long reamer having but slight taper, the diameter can be produced with great accuracy by noting how far the reamer has been introduced into the hole and calculating from the taper.[1] Moreover in case of wear, the tubes can easily be swaged down slightly, and re-reamed. The adjustment is easily made by inserting needles into the tubes and moving till the points touch. The reservoir below the jet may be kept at constant temperature as in the writer's benzol standard, and the air supply may be regulated, as suggested by the writer.

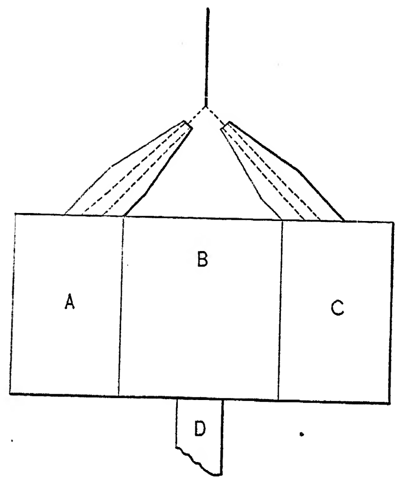


FIG. 1.—Space B filled with wire gauze to prevent wave disturbances. Pressure readings taken at A and C. D, pipe leading to acetylene producer.

(Report to Prof. Houston, Chairman of Committee, Dec. 9, 1893, with respect to the Methven standard.)

Though the work done has been small, two important phenomena have been noted with respect to the acetylene standard.

First: It is but slightly affected by draughts, the effect being small as compared with that on other standards. This is no doubt due to the high pressure at which the gas is burned, *i. e.*, 30.8 cms.

Second: The effect of moist air is much less than that with other standards. This I attribute to the very high temperature of the acetylene flame. Since also the amount of air required to

1. A drift has since been found to be better than a reamer. The drift pin is measured by Dewer and Fleming's optical method.

produce a given candle power is only about seven per cent. of that required with ordinary gas, we see that there is ample reason why the acetylene should be, as my observations show it is, much more independent of atmospheric conditions than other standards.

I regret that at present, lack of facilities has prevented my making quantitative measurements of this gain, but it will be seen from the above considerations that there is good reason to believe it is very great.

In the reports forwarded by the writer to the Chairman of the INSTITUTE Committee during the years 1893-5 some considerations were presented which were omitted in the condensed report, owing to lack of space. As these reports have not been published and as this has led, in at least one case to an unnecessary repetition of work done by the writer, I will give the substance here.

With the exception of those paragraphs enclosed in brackets I have copied the reports as closely as possible.

First: It is not necessary or even desirable that we should have a standard of light accurate to within more than $\frac{1}{2}$ per cent. If we had it we could not use it. In comparing an incandescent lamp with the standard, the spectrum distribution would be different, (unless of course we chose an incandescent lamp as standard, when the same remarks would apply to other sources.) Now, Langley's researches (Trans. *Am. Jour. Science*, Nov. 1888.) show that no two eyes are affected in the same ratio over all parts of the spectrum, and that apparently young people are more sensitive to blue and less to red than older. Consequently if we have two sources, A and B, one A, a rather blue light and B rather red, then one man may record A as much the brighter and another observer may on testing find for his vision the exact opposite to be the case. If these differences were small they would still be important, but when it is pointed out that variations of 50 per cent. are very common and that in some cases estimates differ by 1000 per cent. it will be seen that any great accuracy in the standard is simply useless. Of three observers tested by Langley, one was most sensitive to rays of wave length $\mu = .57$, one to those of $\mu = .52$ and one to those of $\mu = .53$ and from the shape of the curves given it is seen that if these observers had to compare an arc and an incandescent lamp they would have arrived at quite discordant results.

It would seem therefore that what we need is:

(*a.*) *A Standard of Radiation.*—The standard proposed in my report (Oct. 30, 1893,)is a copper ball, thick, blackened, and having a platinum wire wound inside it of such resistance that when one ampere of current is passing through the resistance 4π watts should be radiated from the ball. Or, as an alternative suggestion, that the resistance should be one ohm. I consider the latter the better.

(*b.*) *A Primary Standard of Light.*—This to be the light effect of one watt of D line light. This to be taken as standard and the

spectrum divided into, say, five parts and each compared with the sodium light. (D. line.) The mean of the result of tests on several thousand different individuals, of all ages, to be taken as a provisional optical factor.

Any light source would then be defined as follows, for example, 12/1.2.4.3.2., as regards radiant energy in the visible spectrum, meaning thereby that the total energy in the visible spectrum was 12 watts, of which one watt was in the first section of the spectrum, two in the second, etc. By multiplying the each radiation term by its corresponding optical factor, derived as above, we would get the optical effect, as it would be for the average eye. Represented thus, 3290/90—300—1600—800—500, meaning that the average eye would see the source as possessing a total intensity of 3290, divided into 90 units in the first section, etc.

It is evident then, that the accuracy which we shall need in any general standard of light will depend upon the limits of the probable error in determining the optical coefficients of the various wave lengths. In view of the facts that two eyes may differ in their estimate of the intensity of a colored source compared with sodium light by as much as fifty per cent. I believe that under no circumstances will this probable error be less than $\frac{1}{2}$ per cent., and that this therefore fixes the limit of desirable accuracy. It is of course impossible to say this definitely, as we have not at present sufficient experimental data, but I believe I am on the safe side in my estimate.

(*c.*) Having then a unit of radiation, easily and accurately reproduceable to $\frac{1}{10}$ of 1 per cent., and a unit of optical effect capable of being defined equally accurately, and a set of approximate optical effect coefficients we need now a *set of secondary standards* for practical work. I have suggested the Methven flame and incandescent lamps as such standards. (This was before work on the acetylene standard was begun. This would take the place of the former of these or of both.) These to be used in comparing other sources of unlike spectrum distribution by the use of tinted screens. The secondary standards to be compared with the primary standards of radiation and light.

(The advantage the acetylene standard has, according to the results so far obtained by me, lies principally in the fact that it can be set up anywhere, as a Clark cell can, without resource having to be had to one maker. Its less liability to disturbance from atmospheric conditions and its portability are also valuable, but I regard the question of obtaining a greater accuracy than $\frac{1}{2}$ per cent. by its means to be, while possible, of no importance, for the reasons given above.)

(In the reports a form of differential thermometer was mentioned as being used for comparison of radiations. This method has since been used by Weber and Toepler, but I have in later work found that a massive prism of copper having two thermocouples placed in holes bored in opposite sides of the copper

prism, one couple exposed to the standard, the other to the source to be measured, is better for my purpose. The copper prism is rotatable about a vertical axis, and is slid along the photometer bar till a galvanometer in the thermo circuit comes to zero. On account of the importance of having the black coating of the radiation standard and bolometer of the same nature and thickness it is believed that Lummer and Kurlbaum's method of blackening should be used, as referred to by Messrs. Nichols, Sharp and Matthews.)

(It is obvious that if the sensation of light could be definitely proven to consist of the union of the sensations due to three different wave lengths, as in Maxwell's theory, it would only be necessary to determine the optical coefficients for these three wave lengths and their effect in modifying one another.)

A paper presented at the 13th General Meeting of the American Institute of Electrical Engineers, New York, May 20th 1896. President Duncan in the Chair.

AN ANALYSIS OF TRANSFORMER CURVES.

BY CHAS. K. HUGUET.

One of the most noteworthy features of Prof. Ryan's famous paper on "Transformers,"[1] before this INSTITUTE, was the distortion of the primary current curve on open secondary. This distortion, Prof. Ryan ascribed to hysteresis, and this opinion seems to have been held by Fleming[2], Steinmetz[3], and every other writer since that time, with the single exception of Prof. Rowland.[4]

Dr. Sumpner[5] had previously shown that, assuming hysteresis absent, a variation in the value of μ will superpose on the sinoidal magnetizing current a system of higher harmonics, causing it to become a symmetrically peaked curve. Prof. Rowland, however, went further, and declared that the presence of the distorting harmonics is due, not to hysteresis, but to change in permeability, the effect of hysteresis being capable of representation as a simple resistance. However, he simply expressed this theory mathematically, without giving any experimental verification, and it is doubtless for this reason that the results of his paper have not been accepted in their entirety. This apparent conflict of opinion seemed to warrant further investigation, and it was the object of experiments conducted by the writer at Tulane University in June, 1895, to throw light on this question by analyzing the current curve into its various components.

1. Ryan: "Transformers"; TRANSACTIONS, vol. vii. p. 1.
2. Fleming: "Alternate Current Transformer," vol. ii. p. 454.
3. Steinmetz: "On the Law of Hysteresis (Part III)"; TRANSACTIONS, vol. xi, pp. 731, 743.
4. Rowland: "Notes on the Theory of the Transformer"; *Philosophical Magazine*, vol. 34, p. 54. Also: *Electrical World*, July 9, 1892.
5. Sumpner: *Philosophical Magazine*, June, 1888, p. 468.

The low-tension coil of a 40-light Fort Wayne transformer was used as primary, and a run made at 100 volts and 140 ~. The method employed in the measurement of the instantaneous values was essentially similar to the telephone method described by Nichols[1], a galvanometer being substituted for the telephone. A run was also made at 50 volts and 70 ~, and the determination of the watts lost under these conditions, as well as under the preceding ones, enabled us to discriminate the eddy from the hysteresis losses in the usual manner[2]. In this case the total loss at 100 volts and 140 ~ was 55 watts, of which 20.7 watts were due to eddies, and 34.3 watts to hysteresis. If we divide the

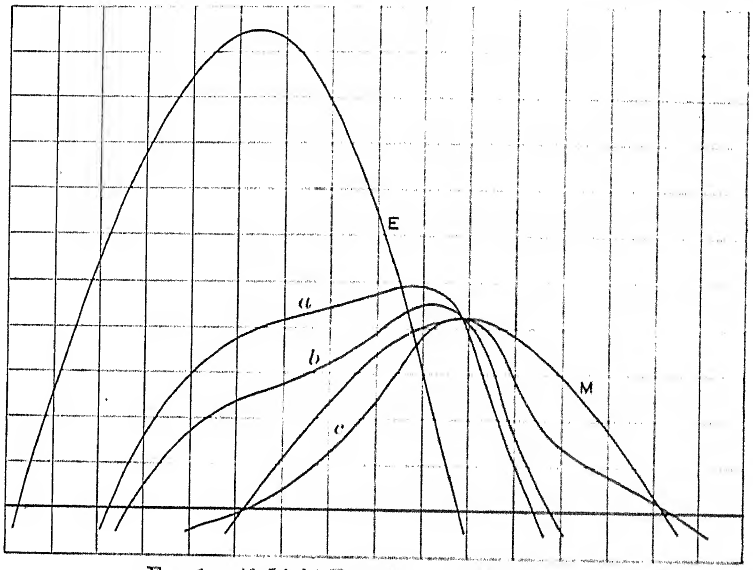


FIG. 1.—40-Light Transformer, 100 v 140 ~.

watts lost by eddies by the E. M. F. applied, we shall obtain the effective current due to eddies, and the ratio of this to the aforesaid E. M. F. is the conductance due to eddies.

If the instantaneous values of E. M. F. be multiplied by this conductance, and the resulting eddy current curve be substracted from the original current curve (*a*, Fig. 1), the remainder will be the hysteresis curve (*b*), *i. e.*, the curve that would be derived from the hysteresis loop. If in the same way we determine the conductance due to both eddies and hysteresis, *i. e.*, if we repre-

1. Nichols: "Laboratory Manual," vol. ii., p. 182.
2. Steinmetz: "On the Law of Hysteresis"; TRANSACTIONS, vol. ix., p. 50.

sent the hysteretic loss as one due to a simple constant resistance, and substract the effective current curve thus determined, from the original current *a*, the remainder *c* will be a true wattless current, since the curve substracted itself accounts for the watts lost. In Fig. 1, this wattless remainder is very fairly symmetrical with respect to the flux curve M, and is of the peaked character to be expected with permeability variable and hysteresis absent. A similar treatment of Prof. Ryan's curve[1] yielded a wattless remainder (*c*, Fig. 2), of similar character. These results seemed to verify Prof. Rowland's theory, at least approximately.

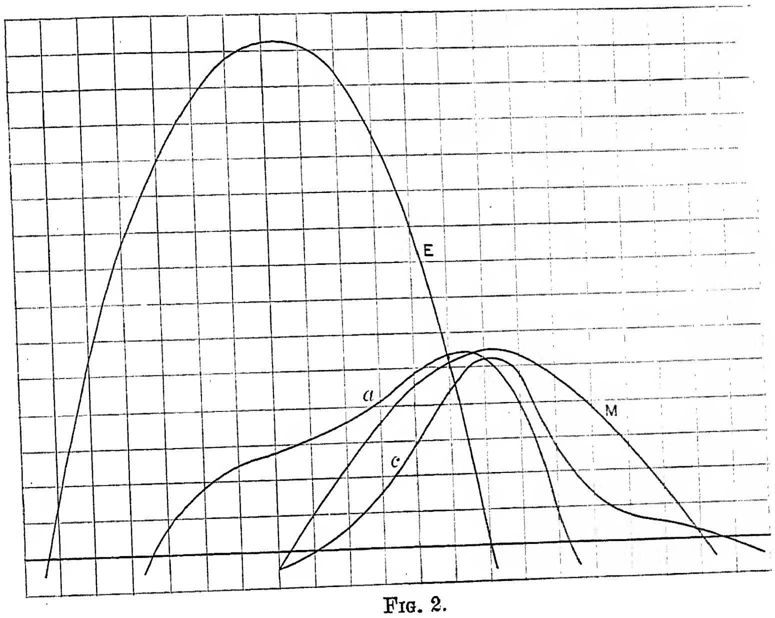


Fig. 2.

But equally strong proof is afforded by Figs. 7, 8, 9, 10 in Steinmetz "On the Law of Hysteresis (Part III.)",[2] for here the subtraction from the original wave, of its equivalent sine wave, leaves a wattless higher harmonic, in each case very nearly symmetrical with respect to the maximum magnetization. Hence these higher harmonics must be due almost entirely to variation in permeability, since hysteresis is essentially unsymmetrical with respect to the magnetization. It is worthy of note that in Fig.

1. Ryan: "Transformers"; Transactions, vol. vii., p. 1.
2. Transactions; vol. xi., pp. 718, 719.

10, in which the original current is most distorted, the wattless higher harmonic is most nearly symmetrical.

The state of the case seems to be, briefly, this: A variable value of μ will superpose upon the wattless sinoidal magnetizing current a symmetrical system of higher odd harmonics, which, since they are symmetrical with respect to the zero of E. M. F., will also be wattless, and the resultant magnetizing current will be a wattless peaked current. If now the transformer be heavily loaded, the sinoidal energy-current will completely hide the magnetizing current both in shape and phase. If, however, the energy-current be comparatively small, we will have a current resultant from a sinoidal current in phase with the E. M. F., and a peaked current 90 degrees behind it, and tending to unduly "boost" the latter half of the energy-current. Hence the dissymmetry is due simply to the conflict between the sine current in phase and the peaked current 90 degrees behind. If either component be very large, comparatively, the resultant will tend to become symmetrical, in the one case a sine, in the other a peaked curve. The sinoidal energy-current may be due to eddies, hysteresis, or secondary load, the effect will be the same.

Curves *a*, *b*, *c*, Fig. 1, show very clearly the effect of the successive elimination from the original current curve of the sinoidal energy-components, due respectively to eddies and the hysteretic loss.

If we take a hysteresis loop, plot a sinoidal flux curve with the same maximum, and then, at each successive epoch, plot the current value corresponding in the loop to the flux value at that epoch, we shall thus obtain the magnetizing current curve. This method was first used by Humphrey and Powell [1] and has been employed by numerous later writers.

If, however, at each epoch, we plot a current value, the mean of those given by the loop for rising and descending magnetization, we shall thus obtain the wattless magnetizing current. This curve is essentially symmetrical with respect to the flux curve, and consequently, as stated, wattless.

If this wattless magnetizing current be substracted from the original current, the remainder will be the hysteretic current curve, or, what is the same thing, we may plot at each epoch the half-width of the loop at the corresponding flux. A curve

1. Humphrey and Powell: "The Efficiency of Transformers"; TRANSACTIONS, vol. vii., p. 311. Also *Electrical Engineer*, vol. x., 1890, p. 16.

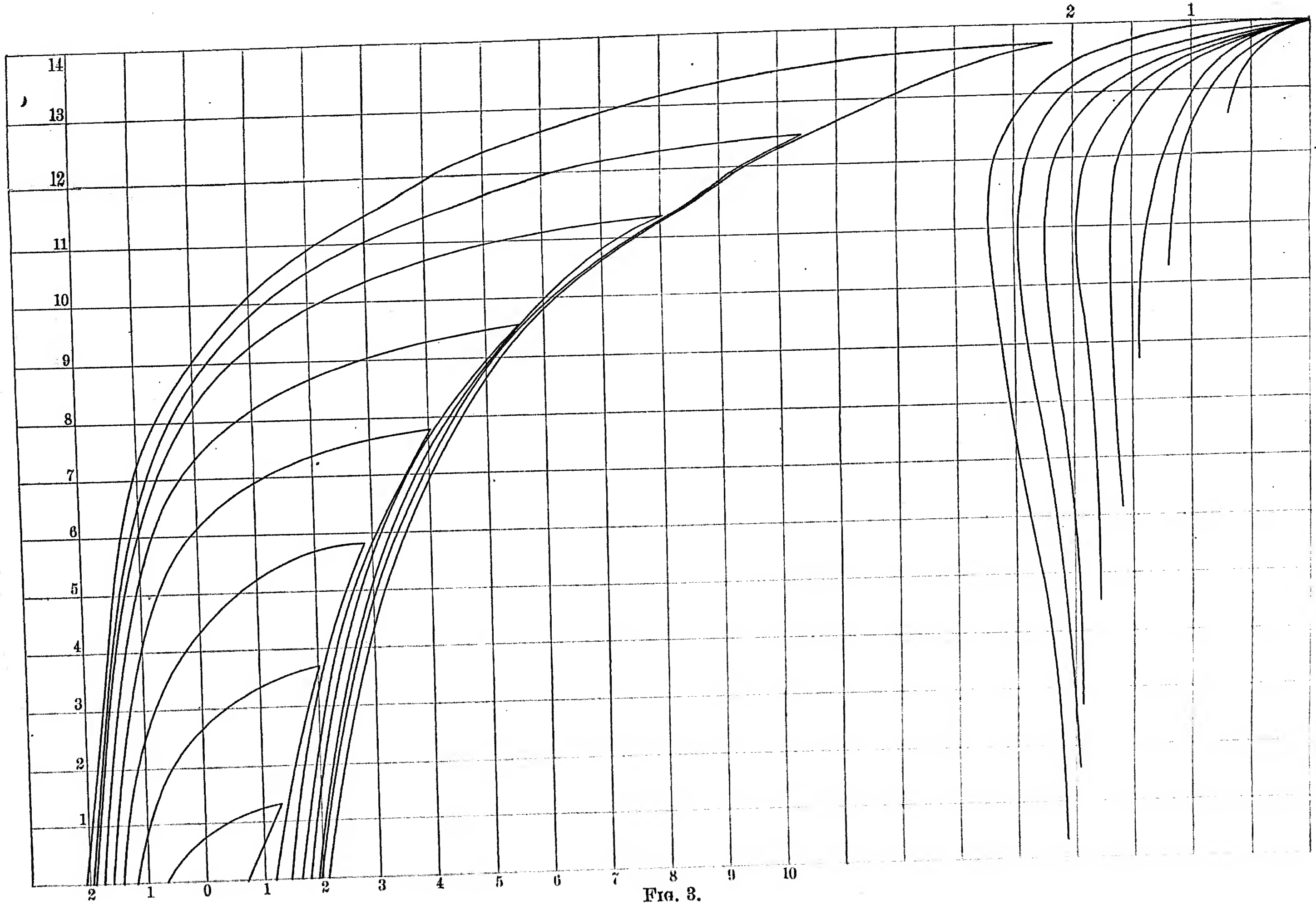


FIG. 3.

plotted between the flux and the corresponding half-width of the loop may be called a *hysteretic characteristic.*

Fig. 3 gives a set of half-loops from Ring III. in Ewing and Klaassen on the "Magnetic Qualities of Iron,"[1] with the corres-

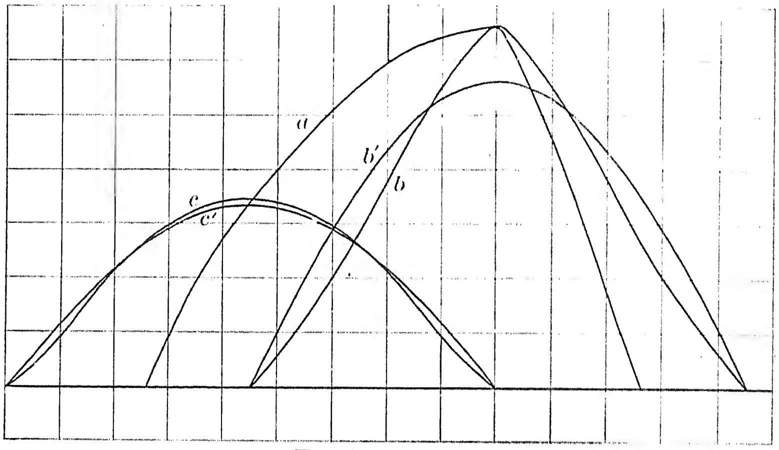


FIG. 4.—B = 1360.

ponding hysteretic characteristics, plotted with their maxima together for convenience of comparison.

Figs. 4, 5, 6 give the magnetizing current curves for sinoidal flux curves, for $B_{\text{max.}}$, 1360, 3720, and 5830, respectively, derived

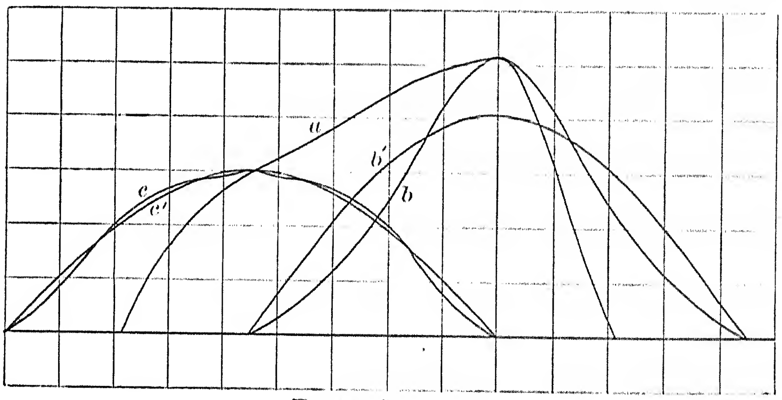


FIG. 5.—B = 3720.

from these loops in the manner above described, with additional curves derived by analysis of the first. In each figure, *a* is the

1. Ewing and Klaassen: "Magnetic Qualities of Iron"; *Transactions of the Royal Society*, vol. 184 (1893), p. 998.

original magnetizing current curve, b the wattless magnetizing current, b' its equivalent sine, which represents the flux curve in shape and phase, c the hysteretic current, and c' its equivalent sine.

The half-width of the loop is zero for maximum flux, and for any flux has the same value, but opposite sign for increasing and decreasing magnetization. Hence the hysteretic current has the same zeros as the E. M. F. curve, and is essentially symmetrical with respect to it, consequently essentially in phase, but it remains to determine the conditions under which it also has the sinoidal form.

In Fig. 5, for $B_{\text{max.}} = 3720$, c lies very close to c', having, indeed, the same maximum.

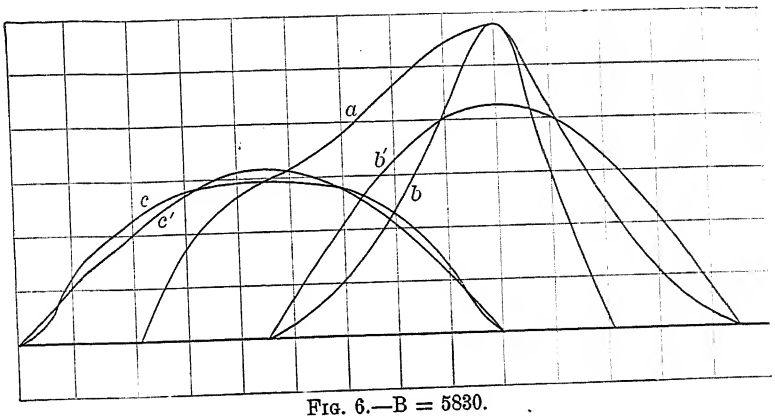


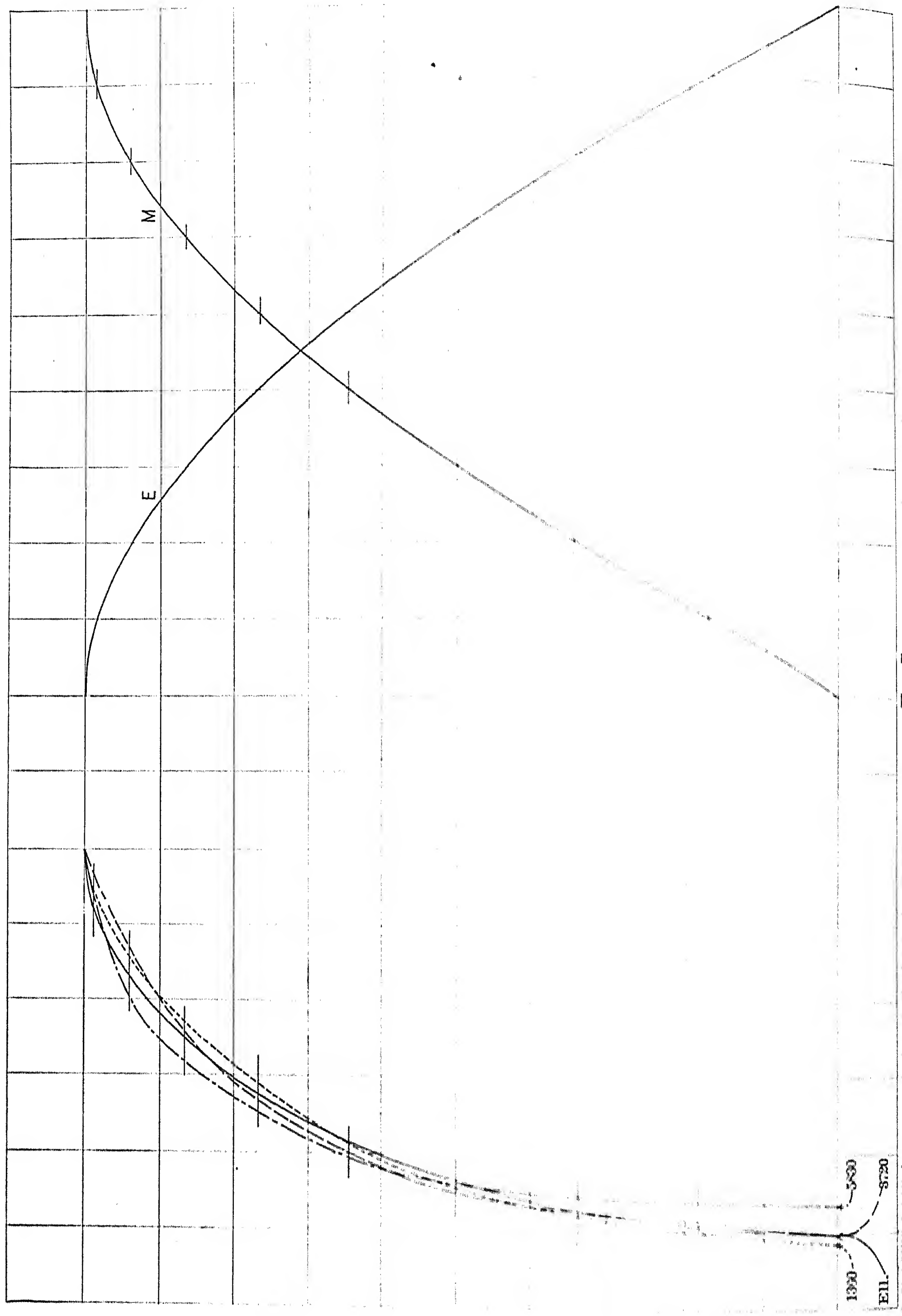
FIG. 6.—B = 5830.

In Fig. 4, for the lower range, 1360, the approximation is very close, but c is slightly more *pointed* than the sine.

In Fig. 6, for the higher range 5830, the approximation is not quite so close, but c is more *flattened* than the sine.

These approximations are quite close, but it is easy to determine the exact form of hysteretic characteristic necessary for a sinoidal hysteretic current, when the flux curve is sinoidal.

Let E and M (Fig. 7.) be sinoidal E. M. F. and flux curves, respectively, drawn to same size for convenience. Assume the hysteretic current due to a simple resistance, *i. e.*, $H = \frac{E}{R}$ (instantaneous values.) Then plot a curve between the flux value at each successive epoch and the hypothetical hysteretic current at that epoch.


M
E
Ell.

A hysteretic characteristic of the form thus determined will obviously satisfy the conditions.

This curve is the quadrant of an ellipse.

For $E^2 + M^2 = A^2$, where $A = E_{\text{max.}} = M_{\text{max.}}$ But $H = \frac{E}{R}$ by assumption, whence $H^2 R^2 + M^2 = A^2$, which is an ellipse, H and M being the variables.

For our purposes the quadrant tells the whole story, so that we may say that the assumption, for sinoidal E. M. F., of hysteresis as due to a simple resistance, is absolutely correct when the hysteretic characteristic is a quadrant of an ellipse.

If we compare this ideal curve with the hysteretic characteristics for the ranges used in Figs. 4, 5, 6, plotting each characteristic to a horizontal scale proportional to the virtual value of its hysteretic current, as indicated by its equivalent sine, we see the same manner and degree of approximation as in the cyclic curves. The ellipse is intersected by horizontal bars indicating the successive epochs in the cyclic curve, and by reference to these it is seen that each characteristic intersects the ellipse at a point corresponding exactly to the intersection of the cyclic hysteretic current by its equivalent sine, and that the relationship is in every respect identical.

A glance at the hysteretic characteristics in Fig. 3 shows that for ranges higher than 5830, the hysteretic current curve will not only be more flattened, but actually depressed in the middle. But while the increased distortion of the hysteretic current will introduce higher harmonies, yet the rapid diminution in the permeability on approaching saturation will introduce them much more rapidly with increase of the range, so that the distorting influence of the hysteretic current will be comparatively slight even at the higher ranges. At the highest ranges, as the one given by Steinmetz[1], mentioned above, $B = 16{,}000$ the inordinate peak of the wattless magnetizing current throws into oblivion higher harmonics from any other source.

To conclude: for sinoidal E. M. F.;

(1) Hysteresis may in all respects be replaced by a constant resistance if the hysteretic characteristic be the quadrant of an ellipse.

(2) This condition is approximately satisfied for moderate ranges of magnetization (such as are used in practice), in reason-

1. TRANSACTIONS, vol. xi., p. 719.

ably good iron, and the higher harmonics are negligible. There will usually be a particular range that will give a nearly perfect approximation. On contraction of the range, the hysteretic current becomes more pointed than the sine, and on expansion beyond the critical range, more flattened than the sine.

(3) The increased distortion of the hysteretic current for higher ranges will cause an increase in the higher harmonics, but the rapid diminution in the permeability on approaching saturation will cause a much greater increase in these harmonics, so that even at higher ranges the distorting influence of hysteresis is comparatively slight.

As a final conclusion, then, we may say that Prof. Rowland's hypothesis that the higher harmonics in the transformer for sinoidal E. M. F. are due, not to hysteresis, but to variation in permeability, and that the effect of hysteresis may be represented by a constant resistance, is approximately correct, for reasonably good iron, is very nearly correct for the moderate ranges used in practice, and may, under certain conditions, be absolutely correct.

A study of hysteretic characteristics would no doubt shed some light on the phenomena of magnetism, but it is beyond the scope of this paper.

Discussion.

Mr. A. E. Kennelly:—It seems to me that this paper is capable of having its principal result summed up in comparatively few words. If the hysteresis cyclic diagram or loop in the case of iron enclosed no area, but consisted of a pair of lines immediately side by side, there would be no energy required to magnetize the iron, and the permeability of the iron would simply change as the magnetism altered. There would then be harmonics in the current wave curves, but there would be no dissipation of energy in the iron. The paper says that if you take the energy which is due both to hysteresis and to eddies, and suppose that the energy is expended just as in a resistance, that is by a current in phase with the electromotive force—if you could imagine that current to exist—and then subtract that current from the current actually observed, you will find a nearly symmetrical curve at right angles to the electromotive force. That is a question which does not conflict, it seems to me, with the existing theory of hysteresis in transformers.

Dr. Nichols:—I regret that Prof. Ryan is not present to-day, as I am sure he would be very much interested in this paper. I hope we shall have the privilege of hearing from Mr. Steinmetz, who has contributed so much to our knowledge of this subject.

MR. STEINMETZ:—While I would like to add a few remarks, I must first express my pleasure to have listened again to a paper, which will not be superseded and worthless in a year or two, as other papers read here have become.

The paper first discusses the question, whether hysteresis or permeability is the cause of the distortion of wave shape in transformers. I do not think there is really any difference in the explanation of the phenomenon by hysteresis or by varying permeability. Hysteresis, according to the derivation of the word, means the discrepancy between magnetizing force and magnetism, consisting in a lag of the latter behind the former. Thus hysteresis presupposes or rather represents an unsymmetrically varying permeability, and thus a distortion of the wave shape due to hysteresis is nothing but a particular case of the distortion due to varying permeability. In my third paper on the "Law of Hysteresis," I have shown a number of instances of distortion of the current wave, independent of hysteresis, by varying permeability, or rather permeance, in the case of a magnetic circuit of periodically varying reluctance. Further on, the paper attempts to separate the effect of varying permeability into two components, one, the energy component, representing the molecular magnetic friction; the other, the wattless magnetizing component.

In my third paper on "Hysteresis," I have discussed and carried through this separation analytically, by the introduction of the "effective" or hysteretic resistance. I have, however, operated all the way through with the equivalent sine wave irrespective of the real wave shape. In this respect this paper offers an essential step in advance, by dissolving the real or observed current wave into two components, the energy and the wattless component, and showing that the energy component is practically a sine wave, and that the distortion observed in the total wave is due essentially to the magnetizing component. This is eminently interesting, since it proves the permissibility of representing the loss by molecular magnetic friction by a constant quantity, an effective resistance or effective conductance.

One assumption, however, is made in the derivation of these results, which may be taken exception to. In the hysteretic cycle, the two values of E. M. F. corresponding to the same magnetization are averaged, and this average curve used as the magnetization curve in plotting the magnetizing current, while the difference between this curve and the hysteretic loop is used for plotting the hysteretic energy current. This difference, or the hysteretic characteristic, is found to be approximately the quadrant of an ellipsis.

In Fig. 1 is shown the hysteretic loop corresponding to the curves shown as Fig. 6 in my paper on "Hysteresis," part 3, plotted in percentages of the maximum value, F—M denotes the hysteretic loop, G the magnetizing curve or curve of average

M. M. F. as used by Mr. Huguet, and H denotes the hysteretic characteristic.

There is no inherent reason why the average M. M. F. should be used as magnetizing curve, and not, for instance, the magnetization curve derived by plotting the average of the two values of magnetism corresponding to the same M. M. F., as shown by V in Fig. 1, or directly the observed magnetization curve A, as derived by electro-dynamometer tests or ballistic galvanometer and the method or reversals. Using the curve A for deriving the wave of magnetizing current, we would get the curve A in Fig. 2, which passes steeply through the zero point, and in general shows a distortion just opposite to that of the curve G, used by the writer; and thus the curve A would not make the energy component of current H a sine wave, as shown in Fig. 2.

In favor of the assumption made by the writer is, however, a feature, to which I drew attention some time ago, namely, that the initial inward bend of the observed magnetic hysteretic A in Fig. 1 is probably an effect of molecular magnetic friction, while the true magnetic characteristic of the material in the absence of hysteresis is a curve similar in shape to the curve G, and very closely resembles the arc of a hyperbola, as shown by Mr. Kennelly in his paper on "Reluctance." When observed under conditions where hysteresis is absent, that is, where the energy consumed by molecular magnetic friction is supplied by some outside source, as for instance an alternating current passing lengthways through the magnetic circuit, the hysteretic loop collapses, and the rising and decreasing magnetic characteristics coincide in the hyperbola. If no energy is supplied from the outside, the magnetization curve at low density cannot follow this curve, since the loss of energy by molecular magnetic friction when rising on this curve would be in excess to the total amount of energy supplied to the magnetic circuit.

That is:

$$\eta\, B_0^{1.6} > \int_{-B_0}^{+B_0} B\, d\, F,$$

where B_0 = maximum magnetization,
B = magnetization,
F = M. M. F.,
η = coefficient of hysteresis.

It would be of interest to investigate this field experimentally, and it could be done probably by winding the core of a magnetic circuit with very fine insulated iron wire, send an alternating current of high frequency through this wire, and then determine the magnetizing current curve of a coil surrounding the magnetic circuit, by the instantaneous contact method, and an alternating current low in frequency compared with the alternating length current in the iron of the magnetic circuit.

If, as it appears from this paper, the hysteretic characteristic is the arc of an ellipsis, while the true magnetization curve is the arc of a hyperbola, the hysteretic loop could be represented analytically by the product of two quartic curves, a result which is very interesting.

As you see, this paper offers quite a number of points of theoretical and practical interest and an ample field for further investigation, and I am therefore very glad that it has been secured for the INSTITUTE.

MR. HUGUET:—In regard to the similarity of hysteresis and variation of permeability, I cannot quite agree with Mr. Steinmetz. If we consider the value of μ in a loop as being dependent on the ratio of the flux to the magnetizing current, then μ would be zero at a and a', infinite at b, and negative from b to a',

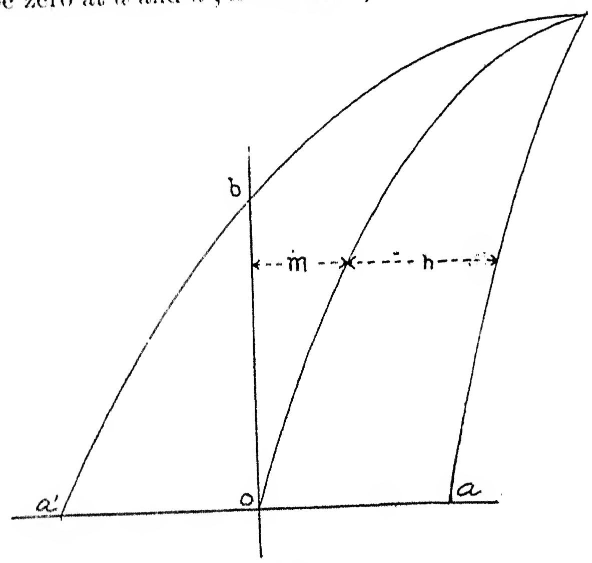


FIG. 8.

all of which we know to be impossible. As a matter of fact, μ should be derived from a curve passing through zero and everywhere intermediate between the rising and descending limbs. Such a curve answers all requirements. This assumption is, in fact, always made, for curves of μ are always single-valued, and hysteresis is either neglected or actually eliminated by vibration or otherwise. If hysteresis is present, as in a loop, the magnetizing current at any flux is then not simply the current required by the value of μ at that flux, but it is necessary to add the hysteretic current. This hysteretic current is in absolute value a function of the flux, the relationship being expressed by the hysteretic characteristic, and its sign is determined by the direction of change of the flux, positive for the rising limb, and negative for the descending. [See Fig. 8.]

Mr. C. T. Rittenhouse:—I think that Prof. Ewing, in a paper presented before the International Electrical Congress, held at Chicago in 1893, made the statement that a distinction must be drawn between the permeable and hysteretic qualities of iron, since specimens showing good permeable qualities need not necessarily possess good hysteretic qualities. Mr. Huguet has made Prof. Rowland the single exception to the theory that upper harmonics are due to hysteresis. In 1894, at the Philadelphia meeting of the Institute, if I am not mistaken, it was this very point which was the subject of animated discussion between Dr. Pupin and Mr. Steinmetz, and although Dr. Pupin did not explain at that time the real cause for upper harmonics, he laid particular stress upon the fact that his experiments showed that upper harmonics could not be due to hysteresis. In reference to this paper, I would like to state that we always hear of closed ferro-magnetic circuit transformers, and yet I doubt if there exists in commercial practice at the present time many of this type. In testing such, it will be found that there is a distinct difference between them and the open core type with which we are more familiar. If we plot the curve relation between the frequency and magnetic induction, using the frequency as abscissa and the induction as ordinates, it will be found that there is a decided drooping of the curve as the frequency increases. This action, I think, cannot be entirely explained by the eddy current theory, if we mean by this the counter-magneto-motive force set up in the iron, due to eddy currents. Starting with this as a basis and expressing the generalized form of Ohm's law as a differential equation, it will be found that the solution of this equation does not satisfy the curves experimentally found. If now even an exceedingly narrow slit is cut in the iron, even as small as .001 of an inch, the drooping of the curves immediately disappears and the relation becomes linear. The explanation of this is doubtless due to the fact that there exists in the absolutely closed core an arrangement of the molecules of the iron in the form of closed chains, and owing to the reluctance with which these chains are broken up, a considerable demagnetizing force is required before a change is effected when suddenly the chain is ruptured and oscillations appear to be set up, which produce distortions in the current wave. If this hypothesis be true, and experiment seems to warrant its assumption, it would indicate that not only hysteresis is to be excluded as a cause of harmonics, but under certain conditions, permeability also.

Mr. C. A. Adams, Jr.:—There is one transformer which was manufactured a few years ago by the Brush Electric Company, of Cleveland, of which the magnetic circuit is completely closed. The core punchings were in disk form, similar to those for armature cores, and the winding was done entirely by hand.

Mr. Ries:—I would state that a closed magnetic circuit was used in the Ries regulating socket—not as a converter particularly,

but as a reaction coil, although I have also successfully used the same construction for converters. The coil was wound by hand, and in order to facilitate the winding, the coil was in the form of a cable containing a number of separate wires.

MR. RITTENHOUSE:—Experiment shows that the magnetizing current for the closed core type is much reduced as compared with the type having a butt joint. This naturally follows from the fact that the reciprocal of the reluctance is directly proportional to the power factor. It would seem, therefore, that the small closed ferro-magnetic circuit transformer, although more expensive to construct, might possess qualities deserving some commercial consideration.

MR. C. A. ADAMS, JR.:—From experiments on butt joint transformers, I have found that the magneto-motive force required for the joints is about five per cent. of the total, in a transformer of fair size with carefully made joints. This percentage is considerably reduced by lapping the plates at the joints, so that the gain in efficiency by using completely closed magnetic circuits is readily seen to be extremely small and not at all sufficient to warrant the extra expense of winding the transformer with closed magnetic circuit.

MR. SCOTT:—It seems to me that the transformers made with lap joint magnetic circuit, approach very close to the absolutely closed magnetic circuit. If two plates of iron be brought together with a butt joint, then the area between the two is equivalent to the cross-section of the plate. If the plate be 100th of an inch in thickness, this may be a very small area. If the plates lap for an inch, the cross-section of the air-gap is a hundred times as great. I agree that there is a marked theoretical difference between open circuit transformers and closed circuit transformer. I also think that there is a corresponding difference in the practical operation of the two kinds of transformers.

MR. RIES:—In reference to the lapped joints of closed circuits, while it is true that lapped joint transformers are much better in general than the open coil or butted joints, yet on the other hand, from a mechanical point of view, the construction becomes more difficult and the space occupied after assembling the parts is very much larger than in the case of closed plates or rings which are cut out of the solid sheet metal and placed closely together. In that way you get a better and more efficient type for a given size and weight. I have come to the conclusion from numerous experiments that I have conducted, that that type, where it can be used, is by far the best.

MR. RITTENHOUSE:—The reason I brought up the question of closed and open ferro-magnetic circuits was not with any intention of advocating the use of the former type of transformer, but for the purpose of pointing out that Mr. Huguet had not conclusively shown that the cause of upper harmonics was in all cases to be attributed to variable permeability.

[Recess until 2 P. M.]

A paper presented at the 13th General Meeting of the American Institute of Electrical Engineers, New York, May 20th, 1896. President Duncan in the Chair.

EFFECT OF TEMPERATURE ON INSULATING MATERIALS.

BY GEO. F. SEVER, A. MONELL AND C. L. PERRY.

In designing electrical apparatus, it is often of the greatest importance to know the effect of different temperatures on the insulation to be used in its construction.

Of the many kinds of insulation, a few of the most common were chosen, *i. e.*, paper, cloth, oiled paper and oiled cloth, and the following conclusions have been drawn from the result of 102 tests on samples of materials, which were kindly furnished by several of the most prominent manufacturers of electrical machinery.

THE APPARATUS.

The heating apparatus consisted of a glass cylinder 8 inches in diameter and 10 inches high, covered at the top and bottom with asbestos plates. The lower part of this cylinder was occupied by twelve enamel resistance tubes, five inches long, and 24 ohms each. The terminals of these were brought out through the asbestos plate. The current in these tubes being controlled by a rheostat, the temperature in the cylinder could be varied at pleasure. An inch above the heating tubes, and supported by an asbestos collar, was a metal plate having holes punched in it to allow the free circulation of air in the cylinder. This plate was connected to one side of a galvanometer.

The insulating material to be tested was wrapped on brass cylinders ¾″ in diameter and three inches long, the insulation not reaching quite to the end of the tube. The insulation was then wound with No. 26 B & S bare copper wire for a

space of $2\frac{1}{2}''$. Five tubes so wound were then placed on the metal plate in the cylinder, and their terminals brought up through the asbestos cover, and then to a switchboard, in order that any tube might be thrown into the galvanometer circuit. This arrangement made it possible to heat five specimens at one time.

With the above apparatus was used a Thomson high resistance galvanometer, a megohm box and a difference of potential of 500 volts.

The thermometer used was of the nitrogen-filled mercurial type and capable of measuring from 0° to 400° C.

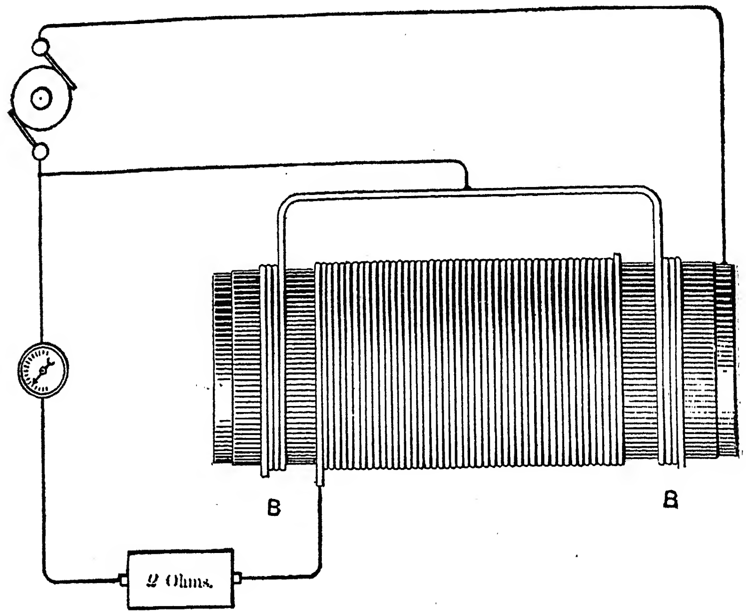


FIG. 1.

When troubled with surface leakage, it was eliminated in the following simple manner. The diagram shows the connections while testing a tube and also the leakage shunt.

It will be seen from the diagram, Fig. 1, that all of the current going through the galvanometer must pass from the dynamo to the brass cylinder, and thence through the insulation to coil A and on through the galvanometer. Any current tending to leak over the surface of the insulation from the brass cylinder to coil A will be intercepted and shunted past the galvanometer by coils B B.

During the investigation many different specimens of paper,

oiled cloth, etc., varying greatly in thickness and composition, were tested, and it was found that each class has its characteristic properties strongly marked. We will now proceed to the discussion of each particular class.

Plain Paper.

In this class 40 specimens were tested. After the resistance at the temperature of the air (22 ° C.) was carefully noted, the temperature was gradually increased (100 ° in one-half hour) and readings taken every 10 ° up to 80 ° and from there on every 20 °. Fig. 2 shows a curve which is characteristic of practically all kinds of plain paper.

It should be noticed however that in general, the resistance of papers that are not protected from moisture, falls between 22 ° and 50 ° and then rises rapidly until at 75 ° it has attained a maxi-

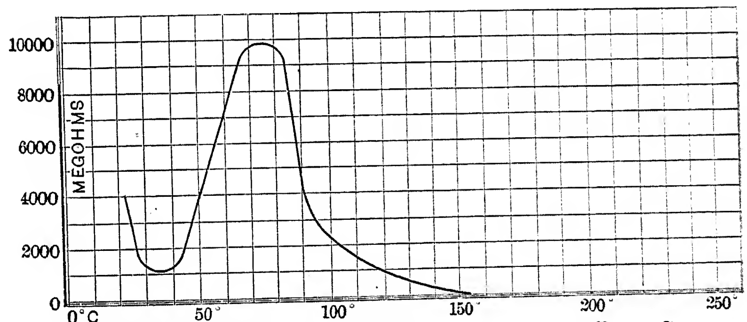


Fig. 2.—Plain Red Paper .009" Thick. Characteristic Paper Curve.

mum resistance. It should also be noted that this temperature of 75 ° is very constant for all kinds of paper. From 75 ° upward the resistance falls rapidly, and at 150 ° is but a small fraction of its initial resistance.

The initial resistance of paper protected from moisture by japan (Fig. 3) is very high, but falls rapidly with the increase of temperature, as is the case with all material protected from moisture. (See oiled paper and oiled cloth). Hence we see that all paper having a porous structure and therefore containing more moisture, has a lower initial resistance than the protected paper, but is affected by heat much less than the latter. This would lead to the conclusion that there are two phenomena taking place. First the driving off of the moisture, which tends to increase the resistance, and secondly, some change (not a mechanical deteriora

tion) in the material, dependent on the temperature and which may be called the temperature coefficient. This temperature coefficient tends to lower the resistance with increase of temperature.

Examining the several curves shown, we see that the initial resistance of unprotected papers is low on account of the presence of moisture. Now, on gradually increasing the temperature, the resistance falls during the first 20 ° or 30 °, because the effect of the temperature coefficient predominates during this period and before the material is warm enough to start the evaporation of the moisture it contains. This however lasts but a short time as the result of the evaporation is to increase the resistance very rapidly, until at 75 ° the temperature coefficient again asserts itself and the resistance rapidly falls.

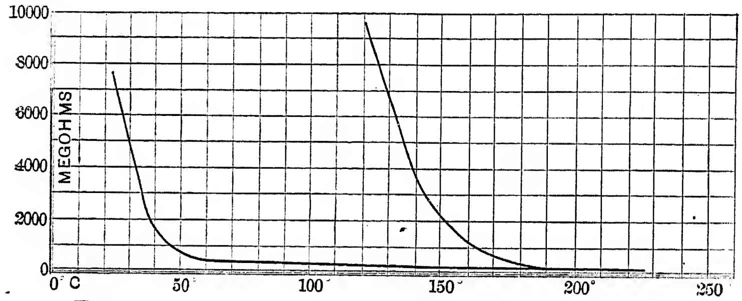


FIG. 3.—Japanned Red Paper .01″ Thick. Characteristic Curve.

In the case of japanned papers (Fig. 3) the initial resistance is very high, due to the absence of moisture, but on being heated the resistance drops rapidly as the small quantity of water contained is evaporated off so slowly that it has very little counteracting effect on the temperature coefficient.

Paper does not seem to deteriorate mechanically at less than 180 °; above this point the material begins to carbonize. At 230 ° a peculiar phenomenon takes place. The material after possessing a very low resistance from 175 ° upward, would at about 230° suddenly increase greatly in resistance and immediately after break down. This may be caused by some molecular rearrangement taking place at that temperature which changes the resistance of the material. Other cases wherein the resistance of a body changes at a certain temperature are well known. In the phenomenon of "recalescence" a piece of iron or steel after

having been heated to a bright redness and allowed to cool slowly will at a certain stage of the cooling process receive a sudden check, heat being generated in the metal as a result of the change which the molecular construction suffers at the critical point. The cooling is arrested and the temperature and resistance rise though the loss by radiation is still going on.

The experiments of F. Kohlrausch and Hopkinson have also shown that the critical temperature is marked by a sudden change in the coefficient which expresses the effect of temperature on the electrical resistance of iron. This is also true of nickel.

It would seem that paper insulation has a critical temperature somewhat analogous to that of iron, steel and nickel, but of course

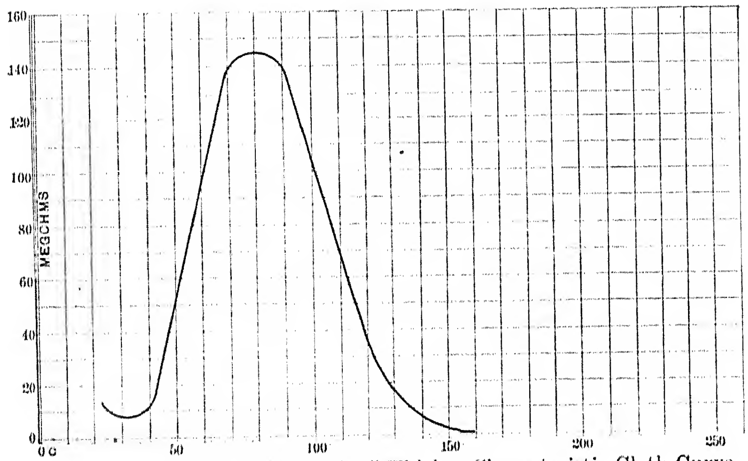


FIG. 4.—Plain Cotton Duck .015" Thick. Characteristic Cloth Curve.

the characteristics of the materials are too different for any close similarity.

PLAIN CLOTH.

Under this head 20 specimens were tested including canvas, linen and muslin, of different thicknesses. The action of this material (Fig. 4) is much the same as that of paper (Fig. 2). The initial resistance is lower as it contains more moisture than is the case with paper; for the same reason when the moisture evaporates off, the increase over the initial resistance is greater than with paper. By reference to the curves of paper and cloth, it will be noticed that their resistance varies in the same manner. For cloth, as for paper, the maximum resistance is at 75 ° C. The

material does not begin to carbonize until at a temperature of 180° C. and even beyond that point it loses its mechanical strength very slowly until past 220 °.

The explanation for the resistance of cloth varying as it does is exactly similar to that for paper.

Oiled Paper.

In this class 14 tests were made, on papers of different thicknesses. With the single exception of one specimen (the resistance of which was very high) the initial resistance was lower than in the case of paper. (Compare Figs. 2 and 5.)

On increasing the temperature the resistance fell rapidly, the curve being much the same as that for japanned paper. The

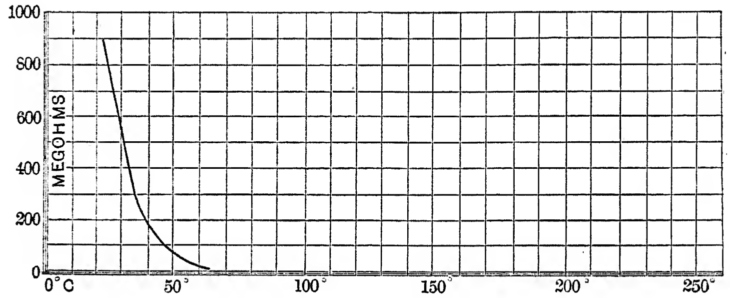


Fig. 5.—Oiled Paper .0045″ Thick. Characteristic Curve.

reason for the sudden decrease in resistance is the same as that for japanned paper.

Oiled paper deteriorates mechanically at a lower temperature than paper or cloth, commencing to blacken at so low a temperature as 120 ° C.

Oiled Cloth.

In this class 28 specimens of oiled silk, muslin, and linen of various thicknesses were tested. The initial resistance of this material is much lower than that of paper, and on increasing the temperature the resistance falls rapidly, the shape of the curve (Fig. 6) corresponding to those of japanned and oiled paper. The reason for the sudden decrease in resistance is undoubtedly the same as for the japanned and oiled paper. The insulation begins to char at about 120 ° C.

GENERAL CONCLUSIONS.

In the foregoing discussion there are some main points to which it is necessary to draw attention. These are:

(*a*) That paper is a better insulation and withstands increase in temperature much better than cloth (shellac and varnish were not used in any of the experiments), oiled paper or oiled cloth.

(*b*) That paper and cloth have a maximum resistance when first heated at about 75 ° C. and are not injured mechanically under 180 ° C.

(*c*) That the point of maximum resistance for paper and cloth (in this case 75° C.) depends on the rapidity with which the temperature is increased.

Here the authors would like to suggest that if the material were kept at a constant temperature until all the moisture had

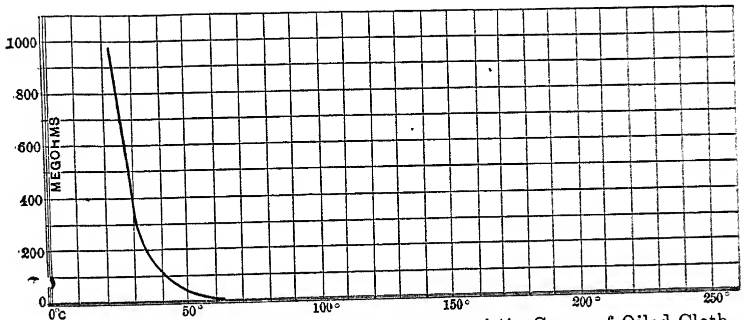


FIG. 6.—Oiled Silk .007″ Thick. Characteristic Curve of Oiled Cloth.

been evaporated, the resistance of the material would then be its *true* resistance at that temperature.

(*d*) That all give a high resistance after cooling, but have little mechanical strength.

Oiled paper and oiled cloth, however, after having been heated to 220° C. and allowed to cool, not only have a high resistance, but became so firmly fixed to the brass cylinders that it was found necessary to remove them with a file.

(*e*) That it would be well to bake paper and cloth insulation to 140° C. before applying varnish or shellac.

(*f*) Referring to Fig 3, it will be seen that on decreasing the temperature the resistance increased, but the second curve does not by any means coincide with the first. On further experimenting in this direction, it was found that there is no temperature at

which the curve with falling temperature coincides with the first. This may be due to more moisture being driven out at higher temperatures which is not absorbed by cooling.

Here it may be well to mention an interesting phenomenon that occurred in connection with this investigation.

When the temperature has risen above 100° C., the zinc in the brass cylinder begins to leave its surface and combine with the copper wire wrapped about the insulation so that the wire has a brass surface and the brass cylinder a copper surface.

Owing to the limited time at the authors' disposal, they were unable to further pursue this investigation, but much remains, and any research along this line will be found full of theoretical as well as practical interest.

Discussion.

Mr. Steinmetz:—While this paper is very interesting, I fear some misleading and very dangerous conclusions may be drawn from it regarding the value of different insulating materials.

The paper shows that dry fibre is superior in insulating quality to oiled paper, or oiled cloth. While it is undoubtedly true and has been observed repeatedly that dry fibre has a very high insulation resistance, and that consequently the insulation resistance is decreased by treating the fibre with oil or japan, high insulation resistance is not the only quality to be sought after, and not even the most important one, but much more important is reliability, and in this regard wood fibre ranges very low, since it is hygroscopic, and thus absorbs moisture. Therefore during the variations of temperatures and humidity to which electric machinery is exposed in practical operation, dry fibre as insulating material is fairly sure to become moist, and thereby very dangerous.

The treatment with oil or japan is merely for the purpose of eliminating the hygroscopic quality of the fibre thereby making it a suitable insulating material.

Besides high insulation resistance, however, another quality has to be considered, namely, disruptive strength, that is the ability of the material to resist being pierced by the voltage to which it is exposed. In medium potential circuits, as for instance, the primaries of alternating circuits, disruptive strength is generally of much greater importance than insulation resistance, and as I have shown in a previous paper, these two qualities do not always go together, but high disruptive strength may be combined with comparatively low insulation resistance, as in mica, or inversely, as in air. In disruptive strength, pure mica ranges above fibre and all other organic substances.

When coming to very high voltages, as in long distance transmission, still another phenomenon becomes of importance, which again changes the relative value of insulating materials, namely, surface leakage. Mica, for instance, while very good in disruptive strength, is not very satisfactory to guard against surface leakage, but a high potential will leak or creep over quite a long distance of mica or porcelain surface, while organic compounds are frequently much superior in this respect.

Thus insulating materials cannot be discussed generally in their relative value, but only with regard to the practical application they have to meet.

DR. F. B. CROCKER:—I agree that the paper standing alone might give rise to the dangerous conclusions of which Mr. Steinmetz has complained, but it was intended simply to investigate certain facts, as the paper itself clearly shows, if carefully read. Investigations were carried on several years ago at Columbia, by T. T. P. Luquer (*Elec. Eng.*, Dec. 28, 1892) showing the effect of moisture on these various materials, and also by Canfield and Robinson (*Elec. Eng.*, Mar. 28, 1894), showing their disruptive strength. It would, of course, be well to put all these together, and a proper comparison would include all these qualities, due weight being given to each. But it is surprising—and I think it not generally known—that the effect of oil appears to diminish the insulating qualities, particularly as soon as the temperature rises. Comparing these curves, we find that even as low a temperature as 50° C. causes the oiled paper and cloth to fall to a very low value, whereas the plain paper and plain cloth have their maximum insulating resistance, which is a very high value, at 75° C. That certainly is very striking and well worth noting. I should also call attention to the fact that probably on the continued application of any of these temperatures there is a greater effect than with the comparatively short time of treating which was the case in these experiments. Therefore, I should expect, that 50° or 60° C. applied for a long time to oiled paper or oiled cloth would bring their insulating resistance down to such a low value that they would be in danger of being broken down. I think that would explain a great many of the gradual burn-outs that occur in electrical machinery; and it remains to be seen by practical experience what the actual facts are. I know that I was very much surprised when I saw the results. I fully agree with Mr. Steinmetz that the insulation resistance is by no means the only point to be considered, nevertheless it is an important one. Now, if these materials do have a very low resistance at such a moderate temperature as 50° or 60° C., why, it would simply mean that they are not suited to work continually at that heat, and 60° C. is not now considered high. Exactly what is the best thing to do is not, of course, the intention of this paper to show, but it would indicate that we ought at least to consider this point, and that if the machines are to be worked

at a high temperature, we should make sure whether oil is a desirable thing to have. In regard to the effect of moisture, that only applies to the starting up of the machine, because when the machine gets to working, that objection disappears. I might add that the tension used in these tests was 500 volts, which is not a very high potential but is sufficient to show any very great tendency to disruption.

Mr. Scott:—I think it is well to put some safeguards and danger signals about a paper of this kind, for taken directly on its face, it is apt to lead to erroneous conclusions. I am familiar with quite a number of tests made by Mr. C. E. Skinner, of the company with which I am connected, on very much the same lines. In his tests he has taken larger pieces of insulating material and placed them between two pieces of iron which were heated up at a slow rate, and the insulation resistance of the material was measured from time to time. The law which he has determined, if expressed on, say, curve 2, of the present paper, has its minimum where this curve has its maximum. That is, in Mr. Skinner's test there is a certain initial resistance which decreases to a minimum at 75 to 100 degrees, and after that increases. The tests to which I refer were made under what were presumed to be fairly commercial conditions in the manufacture of machines for the purpose of determining just what could be expected at different temperatures of different insulating materials. Quite a large number of materials were tested, covering about the range and kind reported in the paper. Now, either those results were wrong or these results are wrong, or they were made under different conditions. I am not prepared at this moment to say which of these is true. I do know that in the tests with which I am familiar, extending over a considerable time, and made in some cases on machines themselves, the resistance of a new machine after successive runs, as the armature or field became warmer and warmer,and also on individual pieces of different kinds of material, the same general characteristics were found in the different tests, disagreeing with those given in this paper.

The subject of oil is mentioned in the paper, and one feature which may explain the rapid decrease in the resistance of the oiled paper may be due to the fact that if there be some moisture in the paper itself and the surface be covered with a film of oil, the moisture will be confined until the temperature is raised, and then it will be vaporized and try to escape. If the surface be nearly continuous, it may contain some openings in the oil. The water will force itself out at certain points; it will pass from a hotter place to a cooler place, and the tendency will be to collect the water at some one point. The moisture, in accumulating in this way, may decrease the resistance very much at one point. Nothing is said in the paper as to the kind of oil used. There are different grades of oil, differing widely in resistance, and

often containing impurities, which may decrease the resistance very much. In the case of some oils, the resistance may be comparatively low. If the oil be heated and then cooled, it may be considerably higher, possibly moisture or impurities in the oil being driven off.

The conclusions given at the end of the paper may be justified from experiments which have been performed, but these, as it has already been stated, cover a small range, and it is going too far to draw conclusions which are general from so limited data. For instance, the first conclusion, that dry paper is a better insulation and stands increased temperature better than cloth. Better for some things, perhaps, but not necessarily for a dynamo. A high resistance galvanometer and laboratory instruments are often dangerous things in practical machinery. Another conclusion of the paper is that it would be well to bake the cloth, etc., at a certain temperature, before applying shellac and varnish. I fail to see the curve or data from which that conclusion is drawn, and I do not understand why that temperature, instead of some other or possibly lower temperature, continued for a long time, may not be a preferable one. I notice on the last page of the paper the statement that when the temperature has risen above 100° C., there seems to be some transfer in the metals which are surrounding the insulating material. It appears from this that the zinc passes from the cylinder through to the wire. If it does, that may have a very decided effect upon the insulation of the medium between the two. If there is electrolytic action, or something similar to that, by which the metal is passed through the insulating material from one side to the other, this phenomena may be very important as well as interesting. The subject is an interesting one, and I call attention to the several points in this way, in order to provoke discussion and prevent any wrong conclusion.

MR. PERRY :—Prof. Crocker called attention to the fact that if paper or cloth were heated, say, to the boiling point of a liquid, you will volatize that liquid. Those of us who have worked much in chemistry know the fallacy of that belief. I remember that in laboratory work we had considerable difficulty in drying some precipitates. Precipitated silica was one of these, and if we had much of it, it seemed almost impossible to drive off all of the water, even though we heated it to a white heat for 24 or 36 hours. In such cases our recourse was to take some small aliquot part of the total, heat this to a white heat, and then after cooling reweigh it. It was then reheated and reweighed, and this was continued until the weighings became constant. We then assumed that the moisture was all out of it. I do not know how it would be with paper, but I should suppose the same thing could be observed there. It seems to me that some of these apparent discrepancies are due to the fact that there is considerable moisture still there, and that that moisture sometimes produced chem-

ical action upon the metals upon which the paper was wound, and in being driven to the outside, carried with it the salts which had been formed. This I should consider as a possible explanation of one of the points referred to in the paper.

PROF. CROCKER:—While Mr. Scott's points are fresh in mind, I would like the opportunity of answering some of them. In the first place I think he should give us the form of those curves. There is a minimum point in the curves shown in the paper, at about 35° C., and we have a maximum point at about 75° C. Then it falls again. If we shift the curve to one side or the other, it would fully explain the statement that Mr. Scott makes. That may not be sufficient, but so far as anything he has said is concerned, it would fully explain the difference. Take, for instance, Fig. 2. The resistance at first is high, then it falls, and there is a minimum point at a little higher temperature. But when the temperature begins to reach a point that evaporates the moisture, then it rises, as I think it must. I cannot imagine that anything else could occur. These materials were taken just as they were received. They were tested on 150 different occasions, and they all gave substantially the same results. They naturally contain a certain amount of moisture which must be driven off at some temperature. Hence, there must be a maximum somewhere between ordinary temperature and a temperature of, say, 150° C., because beyond that we know the temperature will finally destroy their insulating qualities, and we must have those maxima and minima points. It is simply a question of where they occur. I can imagine that under different conditions we might have those minima or maxima occuring at different temperatures, but that general form of curve must necessarily be correct, and anyone would naturally expect substantially these results if he carefully considered the problem. If Mr. Scott remembers the form of those curves, I would suggest that he give them to us, also the temperatures corresponding. In regard to the limited range of these curves, as a matter of fact there were 102 tests made. I think it is very seldom that the INSTITUTE has a paper presented to it which is the result of so many tests as that. I imagine that papers have been read here which were the result of only one test, or perhaps none at all. In regard to the oiled paper and oiled cloth, I would say that 14 tests were made on the former and 28 on the latter; and they were made on materials furnished to us by all the prominent manufacturers in the country. In other words, we took insulating material as we found it. The tests were made entirely without favor. The surprising fact was the uniformity or general similarity that was shown by the different samples of the same class of materials. In regard to the zinc phenomenon, it was observed a year ago; and, while I did not intend to make this statement, as I should prefer to verify it a little more fully, apparently it is a fact that zinc is volatile at quite a low tempera-

ture. A very small amount of zinc vapor is formed in the receiver, and if there is a piece of copper in the same receiver, the latter will absorb the former. That is a very startling statement, perhaps, but after all it is not entirely dissimilar from other physical facts that are well known. Water has a vapor tension down to absolute zero, even ice evaporates. It is by no means necessary to have the temperature at 100° C. in order to have water vapor present. It has a vapor tension at any temperature. Therefore, we might reason by analogy that the zinc also has a vapor tension, and there is a certain amount of zinc vapor present in a space at a comparatively low temperature. It seems to me that the analogy is not carried too far, and it is only necessary to have a small amount of zinc vapor present in the receiver at any given moment, in order that the transfer may slowly occur, and during that time a superficial deposit of zinc is produced. We intended to carry the experiments further before making a positive announcement on the subject, but the discovery crept into this paper accidentally.

MR. SCOTT:—The form of the curve to which I referred is in general a uniform decrease from ordinary temperature until a temperature about a boiling point is reached, the resistance falling off fairly uniformly, and after that increasing, making a fairly smooth curve at the point of minimum resistance. After that time the resistance is under a temperature of, say, 125 degrees to 150 degrees centigrade, as I recollect the curve. There was no falling off—at any rate, no falling off so marked as that shown here in the paper. The first dip in the curve as presented in the paper, as seen in Fig. 2, seems to indicate the presence of moisture which is driven off, that moisture being driven off at about 40 or 50 degrees centigrade. If that lowering of the grade is due to the driving off of moisture, the driving off has occurred much sooner and at a lower temperature in the curves here than in the curves to which I referred. That may be due to the condition of the apparatus. In the tests described in the paper, the specimens are small in amount and entirely surrounded by warm air and have great facility for throwing off moisture. In the tests to which I referred, the material was more confined—more as it is in the insulation of a machine, and the moisture does not have as great facility for passing away.

The special tests upon materials were made on somewhat the same conditions. The iron plates were about eight inches in diameter, so that the insulating material in between was fairly well confined. This may account for the difference in the temperature at which the minimum resistance occurs. After the moisture has passed off and resistance increased, the maximum is found in the curve given in the paper at something under 100 degrees, and the resistance then falls off rapidly, possibly as stated there, to a temperature above 50 degrees. At 150 degrees the resistance is at its lowest point. In the other work to which

I referred, the resistance at that temperature was as high or higher than it was in the beginning of the test. I am sorry that I have not some of these curves and more definite data in regard to them here, but will furnish some curves to be placed with this discussion later. In regard to the number of tests, I grant that 102 is more than are often supplied with the papers. Very often papers are furnished on no tests whatever. But I draw attention again to the statement that I made before, that although there are many tests here in number, they do not cover a very wide range in condition, and yet some rather broad general conclusions are drawn from them. With regard to the transfer of zinc, I inferred from the paragraph in the paper that the zinc left the cylinder under the insulating material. If it did not, but was volatized from the ends or interior of the cylinder, or some other part, then that phenomenon is disposed of.

Mr. Kennelly:—I think, if we appreciate the character of this paper, which is a very interesting and instructive one, we are not likely to form any unfair or unwarranted conclusions from the results which it shows. The subject matter is essentially a practical one, and can only be dealt with, it seems to me, from a practical point of view. The samples before us represent cellulose in a certain condition, containing certain additional materials of a coloring nature, and also additional materials of an oleaginous or varnish nature. It is also impossible to state what the specific resistance of such a material should be, except under so many incidental conditions, as to render the result, when stated, of very little practical importance. The material is fibrous, loose in character, and the structure is capable of absorbing a comparatively large volume of moisture. The resistance of thin films of this substance is given in the curves before us, under certain practical conditions. Their true resistivity is out of the question The material contains water. Those of us who have experimented with condensers know how difficult it is to free paper entirely from the last trace of moisture. It has an absorptive power for moisture in the atmosphere which is only equalled by the avidity by which ammonia will be absorbed by water. The test is made under certain conditions in which a certain amount of water is present, and under a certain degree of pressure between the electrodes The water will be driven off at a certain rate, and the vapor will have a secondary influence upon the results. If you vary the manner in which the experiment is made in any way, it seems to me we should look to have some corresponding variation in the results obtained; but the results, taken as they are and under the particular conditions in which they were measured, are valuable, because we find them at temperatures which are reached commercially. The specific resistances of these materials, while high, are very much lower than they are at normal temperatures. That is, I think, the prominent point, and it is a very important point. Just where the

minimum and maximum will occur in these curves depends on causes so complex that the precise shape of the curve has very little practical importance.

PROF. CROCKER:—While Mr. Scott has explained the general form of that curve, it seems to me that the discrepancy which sounded very great at first, can now be at least partially explained. The conditions in the tests described in the paper were those where the moisture had a fairly good opportunity to escape, whereas in the experiments cited by Mr. Scott the materials were placed between iron plates, and opportunity for the escape of moisture was at a minimum. I should say that the moisture in the latter case was more confined than it is in practical apparatus, because material covered with wire, is a semi-porous mass, through which the moisture can escape, but a sheet of insulating material placed between iron plates has the smallest possible opportunity for the escape of moisture. Consequently, the natural result would be that it takes a higher temperature before the moisture is driven off; therefore, the maximum point of resistance is shifted to the right, because it takes—as Mr. Scott said—a temperature of something like 150° C. before the moisture is all driven off. It simply means that it must be forced off by actual boiling, in contradistinction to mere evaporation. Now, in Fig. 2, the maximum resistance was obtained at about 75 degrees simply because at that temperature the moisture was driven off under conditions which existed in that case, when the insulating material was covered with one layer of copper wire.

MR. STEINMETZ:—It appears to me, Mr. President, that one source of possible error has been overlooked in the paper, and that is chemical action. Most of the varnishes are compounds of organic acids, which are acted upon, and which act upon copper and especially zinc. It is quite possible that the varnish has been acted upon by the zinc, and the results vitiated thereby.

[COMMUNICATED AFTER ADJOURNMENT BY MR. CHAS. F. SCOTT.]

In the discussion of the paper on "Effect of Temperature on Insulating Materials" I referred to certain tests made by Mr. C. E. Skinner. Mr. Skinner has kindly furnished the following statement of some results which he has obtained in his work in this line.

"Some time ago a series of tests were undertaken to determine the effect of temperature on the insulation resistance and breaking down E. M. F. of various insulating materials and of completed apparatus. A large number of insulating materials, including paper, cloth, etc., both in the treated and untreated forms, were tested. The tests on completed apparatus were made on armatures, fields, converters, etc., these pieces of apparatus being treated in various ways before the tests were made.

(1) *Tests on Insulating Materials.*—Various methods of making these tests were tried. The apparatus illustrated in Fig. 1 was finally adopted for standard tests as being best suited for practical work. This piece of apparatus consists of two similar parts, the two parts forming the plates between which the insulating material is placed when tested. These plates are 10 inches in diameter and are carefully surfaced. Each part is made of two separate pieces of cast-iron, fastened together as shown, and wound with a coil of asbestos covered wire. The heating of the apparatus is effected by sending an alternating current through the two coils in series. The heating results from the eddy current and hysteresis loss in the iron as well as from the copper loss in the coil. The rate of heating can be controlled easily by means of a suitable rheostat in circuit with the coils, the mass of iron in the heater being sufficient to prevent sudden changes of

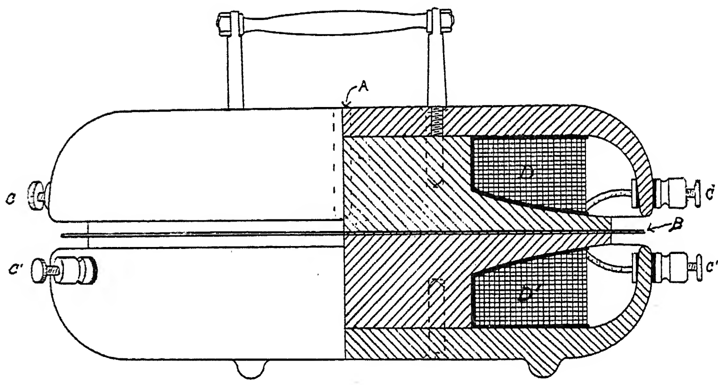


Fig. 1.

temperature from outside causes. The insulation resistance of the samples was measured in all cases by means of a high resistance voltmeter on a 500-volt circuit, all wires of the heating circuit being disconnected when the insulation measurements were made. This method of measurement was adopted on account of the portability of the apparatus and the facility with which the measurements could be made, a complete measurement of insulation resistance requiring from 10 to 15 seconds. Ordinarily the time occupied in making a complete test on a sample of material was from one to two hours. Insulation resistance readings were made at frequent intervals.

In Fig. 2 is given a curve obtained from tests on a piece of untreated paper which is largely used for insulating purposes. As will be seen, the minimum resistance is reached at slightly below 100 degrees C. From this point the resistance rises rapidly until it reaches a point too high to be measured with the

apparatus used. The absolute value of the insulation resistance of any material of this class, depends on the amount of moisture in the material at the start, the rate of heating and the chance for the escape of moisture during the test. The temperature at which the insulation resistance is lowest depends slightly on the

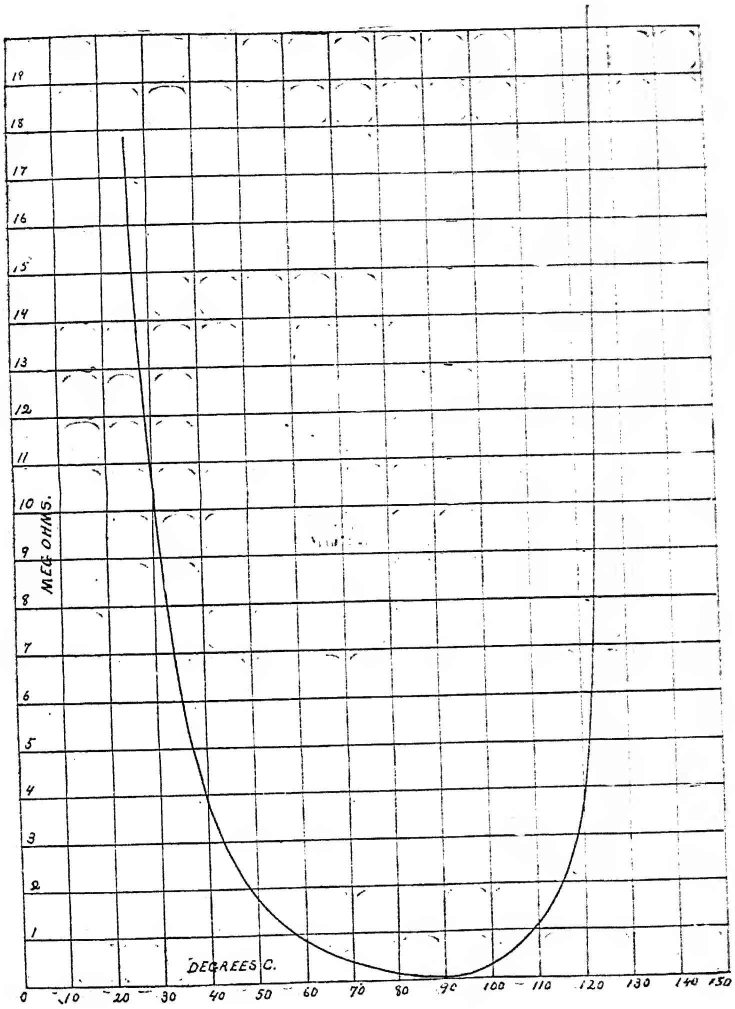


FIG. 2.

rate of heating. The curve shown in Fig. 2 is characteristic of all materials of a fibrous nature which do not contain chemicals or materials which are readily changed by heat. The absolute values of the insulation resistance for different materials or even

for two samples of the same material are rarely the same, but the general shape of the curve has been found to be practically the same for all such materials, the tests extending over a period of several years and being made in many ways.

The curve for treated material, such as oiled-paper, is similar

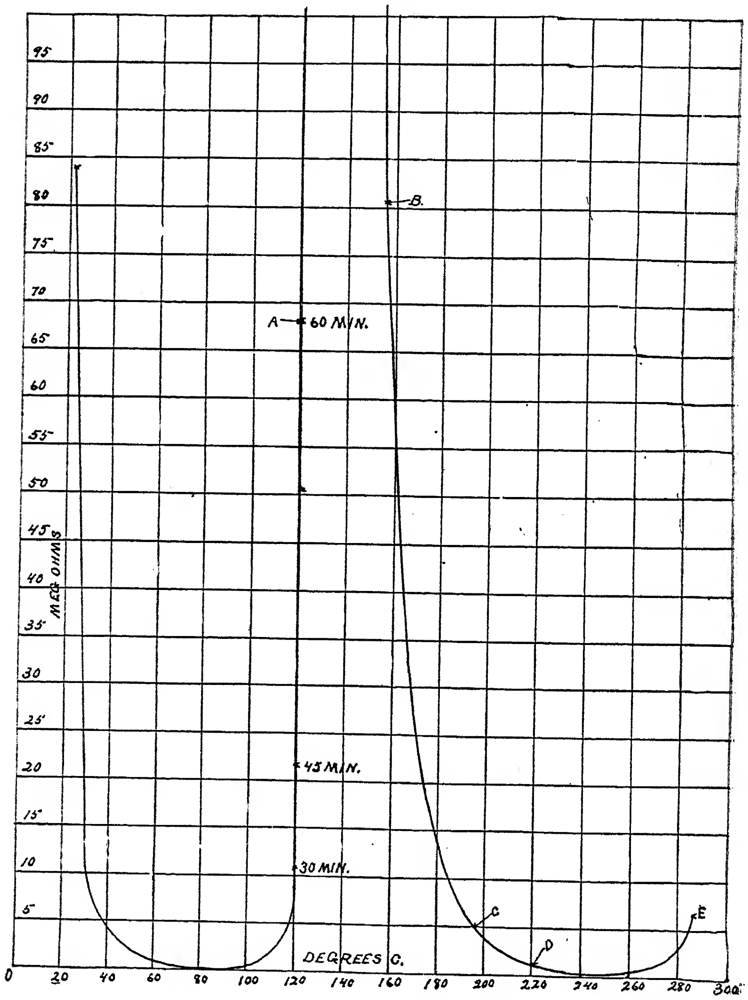


FIG. 3.

to the curve shown in Fig. 2. The absolute value of the insulation resistance depends largely on the drying of the material before treatment. Samples of cloth which were treated with linseed oil after being thoroughly dried and then the oil baked on, do not at any time show a very low insulation resistance, even at a temperature of 150 degrees C.

In Fig. 3 are shown curves for a sample of .015″ calendered press-board. This press-board is made of a mixture of scrap paper and rags. The test on this sample was made as follows: The temperature was gradually raised to 120 degrees C., the time occupied in reaching this temperature being 30 minutes. The

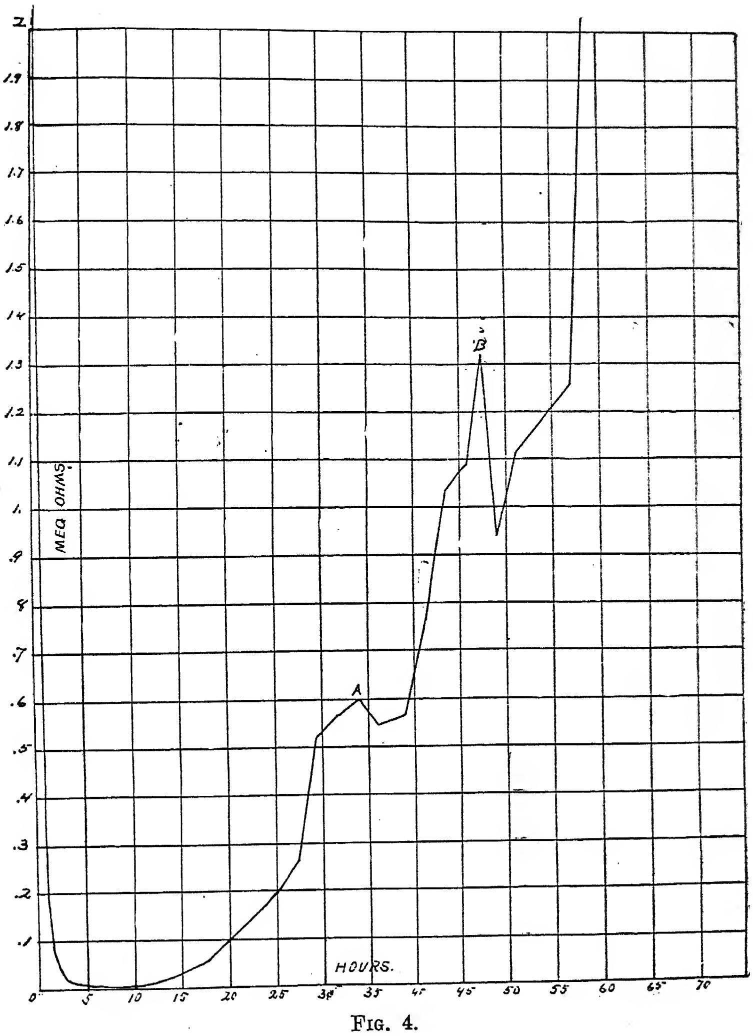


Fig. 4.

temperature was maintained constant at 120 degrees for 31 hours. At the end of this time the temperature was increased to 280 degrees. At the end of 30 minutes the insulation resistance had fallen from 85 megohms to less than one megohm and again

risen to about 12 megohms. The lowest point reached was .032 megohms. At the end of 70 minutes the insulation resistance had become too great to be measured by the apparatus at hand. Frequent readings were taken during the 31 hours' run at 120 degrees C., but at no time was it possible to measure the insulation resistance. When the temperature was again raised at the end of 31 hours, the insulation resistance again fell. At 150 degrees the first measurements could be made. At 195 degrees (point C on the curve) the first smoke was noticed issuing from between the plates. At 220 degrees C. (point D on the curve) smoke was issuing in volumes from all sides of the piece under test. At 280 degrees the sample was charred to such an extent that the edges extending beyond the heating plates were broken off. At this point the test was unavoidably stopped. Sixty hours later, the sample not being disturbed in any way, it was found that the insulation resistance was above 100 megohms.

(2) *Tests on Completed Apparatus.*—In Fig. 4 is shown a curve taken from tests on a motor armature which was wound without previous drying of any of its insulating materials. The motor was run with an overload, and measurements were made at frequent intervals. The temperature could not be taken until the end of the run, when it was found to be about 110 degrees C. The drop in the curve at the points A and B is accounted for by the fact that the load was increased at these points, thus increasing the temperature of the armature. The lowest insulation resistance reached in this test was 4,500 ohms. At the end of 70 hours the insulation resistance on the armature had become so high that insulation resistance measurements could not be made. After standing for 17 days the test was repeated. The general shape of the curve for the second test was the same as for the first, but the insulation resistance did not drop so rapidly nor so low, the lowest point reached being 500,000 ohms. Insulation resistance measurements made on the fields at the same time show that the resistance of this part did not drop so rapidly, but remained low for a longer time. This is explained by the fact that there is a greater amount of wire in the field coils and not so good a chance for the escape of moisture. The curve in Fig. 4 is characteristic of all apparatus tested. The lowest values reached, however, for different apparatus, vary greatly with the character of the apparatus and the previous treatment. Motors which were wound with materials which had been treated then dried, and the motors again dried after being wound, show the best results.

(3) *Relation Between Breaking Down E. M. F. and Insulation Resistance.*—An attempt was made to establish the relation between the breaking down test of insulating materials and their insulation resistance. For this purpose samples of paper which gave a very uniform breaking down test in the normal state, were heated up until the insulation resistance reached a cer-

tain predetermined amount, when the breaking down test was quickly made. It was found impossible to establish any definite relation between the two, even for a given material. It was found, however, that a low insulation resistance usually meant a low breaking down test, but a low breaking down test did not necessarily mean a low insulation resistance. These two tests may be compared to the chemical analysis and the tensile test of iron. A poor chemical analysis means poor physical qualities, but a good chemical analysis does not indicate whether or not there are flaws in the metal.

(4) *Conclusions.*—

(*a*) The insulation resistance of all ordinary fibrous insulating materials, such as paper, cloth, etc., decreases upon being heated up and then increases again when the moisture is expelled.

(*b*) Continued heating of 31 hours at 120 degrees C. does not lower the insulation resistance of paper.

(*c*) The insulation resistance of completed apparatus shows the same characteristics as the insulation resistance of materials taken separately.

(*d*) A low insulation resistance is not necessarily an indication of poor insulation, but probably an indication of the condition of the apparatus in regard to moisture.

(*e*) A high E. M. F. should not be applied to apparatus when the insulation resistance is low.

(*f*) Material which is badly deteriorated mechanically by heat may still have a high insulation resistance, but very poor insulating qualities."

A paper presented at the 13th General Meeting of the American Institute of Electrical Engineers, New York, May 20th, 1896. President Duncan in the Chair.

AN EXPERIMENTAL STUDY OF ELECTRO-MOTIVE FORCES INDUCED ON BREAKING A CIRCUIT.

BY F. J. A. McKITTRICK, WITH AN INTRODUCTION BY DR. EDWARD L. NICHOLS.

PRELIMINARY NOTE.

The method described by Mr. McKittrick in the following paper is one that has been developed by successive steps to meet the requirements of the study of fluctuating electric currents. As long ago as 1880 Joubert[1] introduced what has since proven an extremely useful method in the study of alternating currents, namely, that of instantaneous contact. This method was used independently and almost simultaneously by Thomas and Geyer[2] and has since been developed and perfected by Duncan[3], Ryan[4], Bedell[5] and many others. It has remained in use ever since and has been of the highest service. A variety of methods for the recording of current curves, principally by the aid of photography, have been added. The first attempt in this direction was made by Fröhlich[6] in his well-known experiments with a mirror mounted upon the diaphragm of a telephone. The photographic method has been further developed by Blondel[7] and others. In 1892 Moler[8] constructed a galvanometer, the needle of which had a period so rapid as to make it possible to follow fluctuations, the period

1. Joubert, *Journal de Physique* 1880.
2. Thomas and Geyer; A. I. E. E., vol. viii, p 393, vol. ix, p 263.
3. Duncan, Hutchinson and Wilkes; *Electrical World*, vol. xi, p 160.
4. Ryan; A. I. E. E., vol. vii, p 1.
5. Bedell, Miller and Wagner; A. I. E. E., vol. x, p 498.
(For further references see the last named paper.)
6. Fröhlich; *Elektrotechnische Zeitschrift*, vol. x, p 345.
7. Blondel; *La Lumiere Electrique*, vol. xli, p 401.
8. Moler; A. I. E. E., vol. viii, p 324.

of which did not exceed one one-hundredth of a second. In this instrument, as is well known, the record was made by the movement of a pointer over the surface of a smoked cylinder. Since the construction of the Moler curve-writing galvanometer, Hotchkiss[9] has busied himself with the development of an instrument the needle of which has a much higher rate of vibration. By means of this galvanometer, which is described in the succeeding paragraphs of this paper, a great variety of curves have already been obtained, and its application to the problems studied by Mr. McKittrick is herewith presented to the INSTITUTE in the belief that it will serve to illustrate the usefulness of the method in question.

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Since the days of Faraday, many experimental studies have been made of induction currents. The list of experimenters is a long one including, in Europe, the leading physicists of each generation, while on this side of the Atlantic the researches of Henry have connected his name permanently with electro-magnetic induction. Nearly all the researches have been made by the aid of a secondary circuit. Mr. F. E. Millis has however, photographed the extra current in the circuit itself when the E. M. F. is suddenly removed, and has shown that it has a physical as well as a mathematical existence. (*Phys. Rev.*, vol. iii, No. 17.) One of the earliest forms of circuit by which Faraday illustrated the existence of the extra current of self induction is shown in Fig. 1, in which C is a coil containing iron, such as an electro-magnet, through which a current is flowing from a battery A; B is another circuit containing a galvanometer connected in parallel with C at the points F and G. By blocking the needle of the galvanometer so that it is not deflected by the battery current flowing from F to G, it will indicate, on breaking A, the presence of a current flowing from G to F induced, as we know, in the branch C. That this induced current may exceed in intensity the original current flowing through B may be simply shown by placing in B an incandescent lamp. Under suitable conditions, while the steady current from A is flowing, the lamp will remain quite dark, but on breaking the circuit A, it will flash into incandescence.

The present paper is an attempt to describe the results of an introductory experimental study of the induction phenomena exhibited when the above form of circuit is broken. The results

9. Hotchkiss and Millis; *Physical Review*, vol. iii, p 49; also F. E. Millis, *Physical Review*, vol. iii, p 351.

presented must be regarded as chiefly illustrative, but it is hoped that they may be found interesting, perhaps useful, to the INSTITUTE, since the fields of all dynamos and motors are enormous electro-magnets and at every change of the current through them, E. M. F.'s are developed, a knowledge of which is essential to the electrical engineer.

In the galvanometer for photographing alternating current curves (*Physical Review*, vol. iii, No. 13) which has been brought

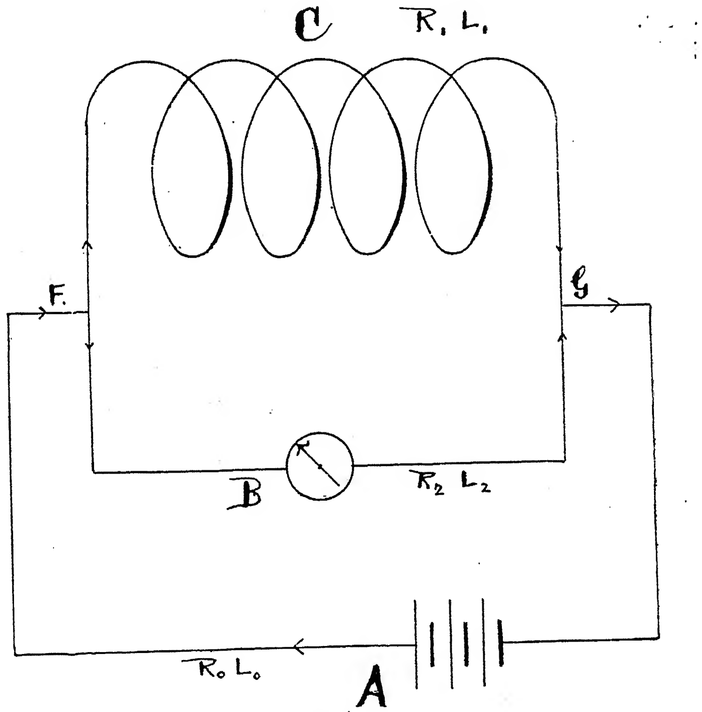


FIG. 1.

to such a high state of perfection by Messrs. Hotchkiss and Millis, we have an ideal instrument for studying such rapidly varying currents as accompany a break in a circuit. It was by the aid of such an instrument that the present work was done.

In the article cited above, the construction of this galvanometer is fully described. Since, however, the form used by the writer was modeled after later designs of Mr. Hotchkiss, it may be well to outline briefly the construction of the principal parts.

The apparatus in question is a photographic galvanometer.

From a minute mirror attached to an equally minute needle, light from some source is reflected to a narrow slit past which a photographic plate is dropped, thus recording any motion of the needle. In the experiments to be described, the needle used consisted of a minute piece of soft iron 1.65 mm. by 1.07 mm. and .07 mm. thick, to which was attached a mirror .53 mm. by .42 mm. made from a silvered microscope cover glass. The needle and mirror are mounted on a quartz fibre. Lengthwise of a brass rod .32 cm. in diameter is cut a slot .16 cm. wide, of equal depth, and about 3 cm. long. At the bottom of this slot and about the middle of its length, a hole, the same width as the slot and .7 cm. long, is drilled through the remaining portion of the rod. To the shoulders thus formed the quartz fibre is attached, suspending the

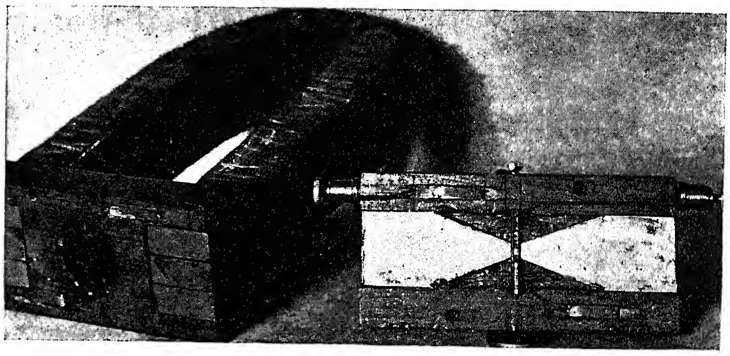


FIG. 2.

needle and mirror in the middle of the hole. This brass rod and the needle suspended in it can be seen in Fig. 2.

From the vibrations of a standard tuning fork (768 vibs) photographed on a plate with those of the needle, the period of the needle is easily found. It is 2656 complete vibrations per second.

The general construction of the galvanometer can be seen from Fig. 2. The permanent horse-shoe magnet has a length of 20 cm. and a total width of 10.8 cm. A block of wood 10.8 cm. by 4.9 cm. by 1.6 cm. slips over the ends of the magnet as shown, while in a second block 10.8 cm. by 4.9 cm. by 2 cm. is a groove 2.5 cm. wide and 1.5 cm. deep, carrying the tapering pole-pieces as shown. The two coils of the galvanometer are fixed by paraffin in holes 2.5 cm. in diameter and 1.7 cm. deep,

bored, one in each block, so that one coil is flush with the surface of the first block, and the other with the bottom of the groove in the second. These two blocks are placed face to face and fastened by screws. The brass wire containing the needle is slipped vertically down between the coils and pole-pieces as shown. Connection between the two coils is made by a spring, the other terminals being connected to binding posts.

The coils of the galvanometer were wound with No. 36 B & S. copper wire on a standard form used by the mechanician of the Physical Department for the construction of the most sensitive galvanometers. The resistance of the coils in series is 270 ohms at 18° C., while the self-induction is so small that attempts at determining it have led to negative results.

The pole pieces mentioned above and shown clearly in Fig. 2, were made of soft iron 2.5 cm. wide and .5 cm. thick. Each was 3 cm. long and tapered to a tip .5 cm. by .2 cm. They fitted tightly into the groove and were adjusted by microscope to equal distances from the needle.

In order to protect the photographic plate from light, the part of the apparatus shown in Fig. 3, was constructed. The vertical slide, in which the plate drops, is 18 cm. by 7.5 cm. and 90 cm. long. In the inside of the slide, two cleats extend the length of the box between which and the back of the box the plate-holder drops One cleat extends only to within 23 cm. of the top of the box in order that the holder may be placed in position. A second box 48 cm. by 18 cm. by 10 cm. containing the galvanometer is attached by one end to the slide, the other end being supported by legs. Where the boxes are in contact, an opening 1.7 cm. by 15 cm. is cut through both, and across this opening on the inside of the slide, is pasted a piece of cardboard having a narrow horizontal slit 12.7 cm. by .05 cm. and through which the reflected light from the galvanometer passes to the plate. Light passes to the mirror of the galvanometer through a vertical slit in a shutter shown clearly in Fig. 3. This shutter is adjustable about a vertical axis at the end of a box.

The galvanometer is so supported that it is adjustable by hand about a vertical axis passing through the needle, while a screw at the end allows delicate adjustments to be made about a horizontal axis. Before dropping, a brass catch holds the plate holder in position from which it can be released at will, while a cushion of waste at the bottom of the slide diminishes the shock of the

drop and a small spring catch prevents a rebound. The whole apparatus is painted a dead black, while felt attached to the doors serves to keep out all useless light.

A small mirror which is attached to the galvanometer itself, and is adjustable, was used to trace out a reference line as seen

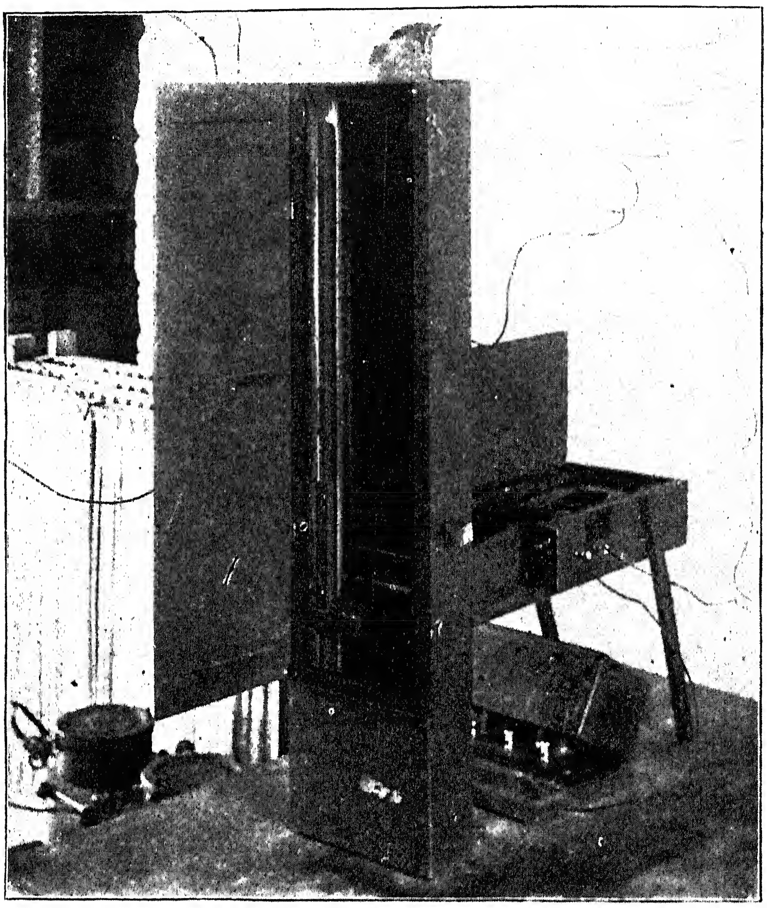


Fig. 3.

on the curves. By it an exact zero line could be obtained but only with great care in adjustment. Since in general an absolute zero line is not necessary, this mirror was usually only adjusted to trace an approximate zero line.

The calibration curve for the galvanometer is given in Fig. 5. The arrangement of apparatus during the investigation is shown

in Fig. 4, in which C is an electro-magnet through which a current is flowing from a battery in the branch A; B represents the galvanometer placed in series with a high resistance in parallel with C at the points F and G; L is the arc lamp, and S the slide in which the plate drops; H is a form of circuit breaker placed in A and controlled by an electro-magnet operated by an auxiliary circuit. When a current is flowing in this auxiliary circuit, the terminals in the breaker are held in contact against the force of a spring and the circuit through C is complete. The plate holder in dropping, operates a trip which breaks this auxiliary circuit,

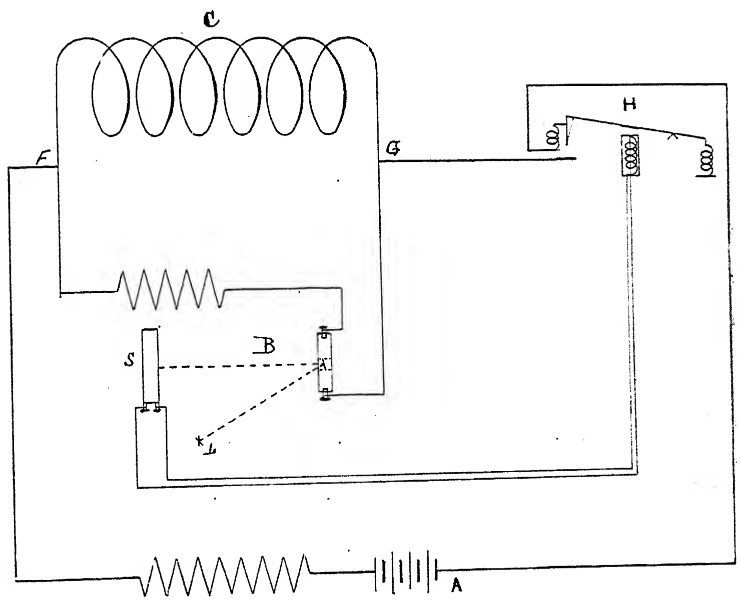


Fig. 4.

and hence the main circuit, the effects of the break being recorded on the plate.

The circuit breaker used, consisted of a modified form of telegraphic sounder. The frame-work which usually limits the motion of the armature lever was removed, and the lever itself lengthened. To the end of this lever, at right angles to it, and in a vertical plane, was fastened the metal forming one terminal, to which was attached a wire carrying the current. This wire was so flexible that it did not materially interfere with the motion of the lever. The upper terminal consisted of a strip of metal

about .04 cm. in thickness, tapering to a width of about .2 cm. The other terminal was a sheet of the same metal about 5 cm. square, placed horizontally beneath the upper terminal.

The source of light used was an arc lamp fed by hand, and enclosed in a box, in the front of which a vertical slit, .5 mm. in width, allowed the light to pass to the mirror. The carbons were

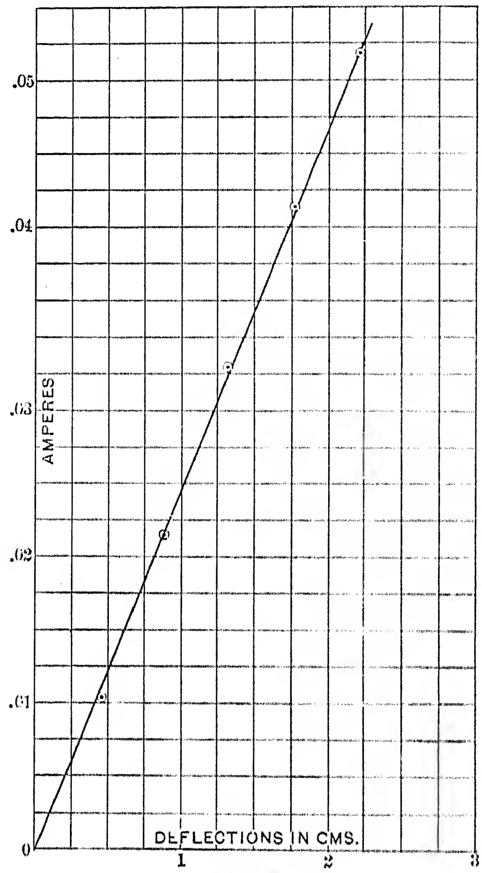


FIG. 5.

tilted at an angle of about 30° in order that the brightest light from the crater might be reflected to the plate.

Since the phenomena which we wish to study occurs when the circuit A is broken, it is well to examine what happens when a circuit through which a current is flowing is opened, and by what law the current falls to zero.

As we open a metallic switch, for example, an arc is formed

through which the current continues to flow until the terminals of the switch become separated so far that the E. M. F. of the circuit is unable to force a current across. The spark then breaks. What actually occurs in the formation and rupture of such an arc is unknown. The nature of the arcs so formed must vary under different conditions of breaking, and the law by which the current reaches zero in each case must vary with the arc so formed and its time of duration.

It has been established theoretically and verified experimentally, that, on the removal of the impressed E. M. F. from a circuit, the current reaches zero by a logarithmic curve. That the current curve under the conditions of an ordinary break may differ widely from a logarithmic curve can be easily understood. That

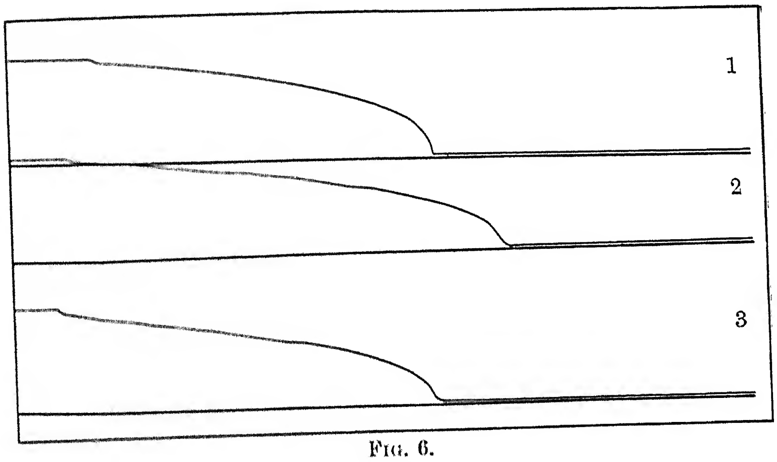


FIG. 6.

such actually is the case, can be seen from Figs. 6 and 7, which show the dying away of the current, when a simple circuit is broken. These curves were obtained by connecting the galvanometer just described, in parallel with a non-inductive resistance of 3.385 ohms, placed in series with the coil *c* used during the present investigation, and breaking the circuit in the manner described above.

In Fig 6, the three curves represent a "break" under different conditions. The curves are traced from left to right. The three straight lines are the three reference lines, approximately zero lines. Beginning at the left, the straight part of each curve represents the steady deflection of the needle, the distance of this deflection from the zero line being proportional to the strength

of the current. The point at which the "break" begins is clearly marked by a slight notch. From that point the curves fall away approaching the reference line quite slowly at first but abruptly at the end. From the point where the "break" ends, the curves run parallel to the reference line.

Fig. 6 (1) is a "break" in which the terminals of the circuit-breaker are of copper; E, the E. M. F. of the circuit is 170 volts, L, the self-induction, .029 henrys and I, the current flowing before the "break," 5.2 amperes. The time taken by the spot of light to trace out any part of the curve is found by comparing the curve with the vibrations of a tuning fork of known pitch photographed on a plate dropping at same speed. Since in all cases the plate holder fell quite freely, it may be assumed that the speed of all plates in passing the slit was the same. The duration of the spark in Fig. 6 (1) is thus found to be .065 second.

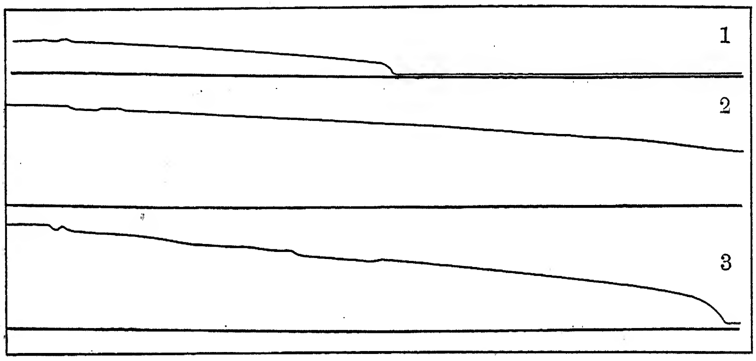


Fig. 7.

Fig. 6 (2) shows a "break" when the breaking terminals were of brass, all other conditions remaining as in (1). The time of duration of the spark is .075 second.

Fig. 6 (3) shows a "break" under the same conditions as (2), except that the spring of the circuit-breaker is strengthened, thus separating the terminals more quickly, and hastening the destruction of the arc. The time of duration of the spark is .068 seconds.

Fig. 7 (2) and (3) show "breaks" with brass and copper terminals, respectively, in which the current is the same as in Fig. 6, but the spring of the circuit-breaker, weaker. The spark in (2) lasts longer than one-ninth of a second, so that its ending is not recorded in the plate. In (3) the terminals are of copper and the spark again ends more quickly than with brass terminals.

Fig. 7 (1) shows a "break" with brass terminals, the current being only 1.5 amperes, while other conditions remain as in (2) and (3).

Fig. 8 (1) and (2) show current curves for a "break" from zinc terminals. The irregularities in the curves are characteristic of all "breaks" from zinc terminals, and seem to indicate that the arc formed is very irregular.

The current curves just discussed can, however, be only accepted as illustrative of one method of breaking the circuit.

It is clear that in order to follow theoretically the instantaneous values of the induced currents in the form of circuit shown in Fig. 1 when A is broken, the current curve during the break should be known. Since, in general, this curve is not known, it

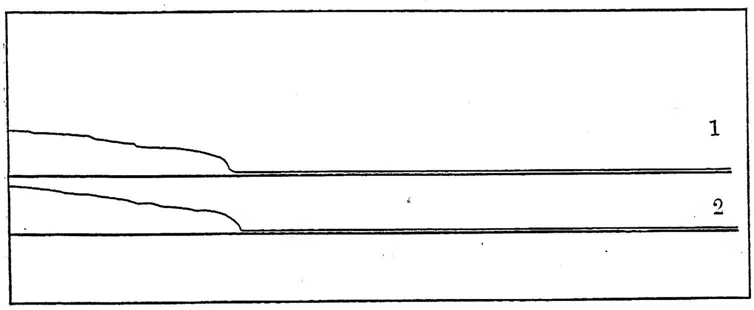


FIG. 8.

is only possible to consider particular cases. Let us examine, then, what occurs under perhaps the simplest conditions which can be imagined.

Consider again the circuit in Fig. 1. Let R_0 L_0 be the resistance and self-induction of the branch A, and let R_1 L_1 and R_2 L_2 have the same meaning for C and B, respectively. Let L_0 be zero, for simplicity, and $L_1 > L_2$. During the break in A, we shall have three sources of E. M. F. in this circuit, viz., the E. M. F. of the battery A, which we shall call E, and the two E. M. F.'s of self-induction; one in C and one in B. Let us think of the current produced in any branch by each E. M. F. as separate from that produced by each of the others. The resultant actual current at any instant in any branch will be the resultant of the three hypothetical currents generated respectively by the three E. M. F.'s. That it is legitimate to think of each E. M. F. as generating its own current independent of the

action of other E. M. F.'s in the circuit, is evident when we consider that the solution of any differential equation for the current in any circuit in which there are several sources of E. M. F. results in an expression involving the sum of different terms, each term representing the value of a current produced by a corresponding E. M. F.

Let the current due to E be I_0, dividing into I_1 through C, and I_2 through B. Let I_1^1 be the current produced by the E. M. F. of self-induction in C, and dividing through the branches B and A, and let R_1^1 and L_1^1 be the equivalent resistance and self-induction of the path of this current. Let I_2^1 be the induced current in B, and L_2^1, R_2^1, the equivalent self-induction and resistance of its path.

Suppose now that when A is broken, the character of the break

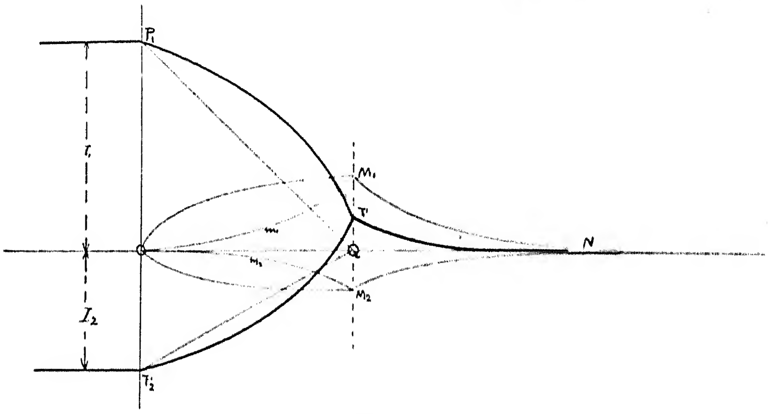


Fig. 9.

is such that I_0 decreases uniformly during T the time of duration of the spark. If we plot currents as ordinates and times as abscissæ in Fig. 9, we may represent the decrease of I_1 and I_2 by the straight lines P1 Q and P2 Q respectively, where O Q represents T, the time of the duration of the spark.

We have during that time T, $L_1 I_1$ lines of force in the field of C, due to I_1, uniformly removed, and hence we have a uniform E. M. F. acting in the circuit C during that time, equal in magnitude to $\frac{L_1 I_1}{T}$. At any instant, then, we have for the instantaneous value of the induced current

$$i_1^1 = \frac{E}{R_1^1}\left(1 - e^{-\frac{R_1^1 t}{L_1^1}}\right) = \frac{L_1 I_1}{T R_1^1}\left(1 - e^{-\frac{R_1^1 t}{L_1^1}}\right),$$

and if R_1^1 and L_1^1 were constant, i_1^1 would increase according to a logarithmic curve.

Now R_1^1 is not even approximately constant. For

$$R_1^1 = R_1 + \frac{R_0 R_2}{R_0 + R_2},$$

and R_0 is increasing rapidly. I_1^1 may be represented, then, by some curve $O M_1$, whose form will vary with the character of the spark, rising to a maximum at the end of the time T, and from the point M_1 dying away according to a logarithmic curve $M_1 N$, whose equation is

$$i_1^1 = i_1^1{}_{\text{max.}} \times e^{-\frac{R_1 + R_2}{L_1 + L_2} t}.$$

Similarly, we can represent the induced current I_2^1 in B by the curve $O M_2$, during the time of the spark, and $M_2 N$ afterwards.

The current through C, at any instant, is the resultant of I_1, and I_1^1, at that instant, and the part of the current I_2^1 which is then flowing through C. Denoting the instantaneous value of this resultant current through C by i_c, we have

$$i_c = i_1 + i_1^1 - \frac{R_0}{R_0 + R_1} i_2^1.$$

From this equation we see that since, by assumption, $i_1^1 > i_2^1$, i_c will always be positive. When $i_1 = 0$, then we have $\frac{R_0}{R_0 + R_1}$ unity, and $i_c = i_1^1 - i_2^1$. From that instant at any time t

$$i_c = (i_1^1 - i_2^1)_{\text{at end of time } T} \times e^{-\frac{R_1 + R_2}{L_1 + L_2} t}.$$

If the curve $O m_2 M_2$ represents the proportion of i_2^1 which flows through C, i_c may be plotted by adding at each instant the coordinates of the curves $O M_1 N$, $P_1 Q$ and $O m_2 M_2$ taken with their proper signs. The resultant curve for the current flowing in C will be $P_1 P N$.

Similarly for i_B, the instantaneous value of the resultant current in B, we have

$$i_B = i_2 + i_2^1 - \frac{R_0}{R_0 + R_2} i_1^1.$$

Since i_1^1 is increasingly greater than i_2^1, this equation shows that i_B gradually decreases and that at some time when

$$i_2 + i_2^1 = \frac{R_0}{R_0 + R_2} i_1^1,$$

i_B is zero. After that i_B is negative and gradually increases to a maximum at the end of the time T. i_2 is then zero, R_0 infinite, $\frac{R_0}{R_0 + R_2}$ unity, and $i_B = i_2^1 - i_1^1$. From that instant i_B decreases to zero according to the equation,

$$i_B = (i_2^1 - i_1^1)_{\text{at end of time } T} \times e^{-\frac{R_1 + R_2}{L_1 + L_2} t}.$$

After the ending of the spark, we thus have i_B equal in magnitude to i_C. If

$$\frac{R_0}{R_0 + R_2} i_1^1$$

is represented by the curve $O m_1 M_1$, i_B will be represented by the curve $P_2 P N$ whose ordinates at any instant will be the sum of the ordinates $P_2 T$, $O M_2 N$ and $O m_1 M_1 N$, taken with their proper signs.

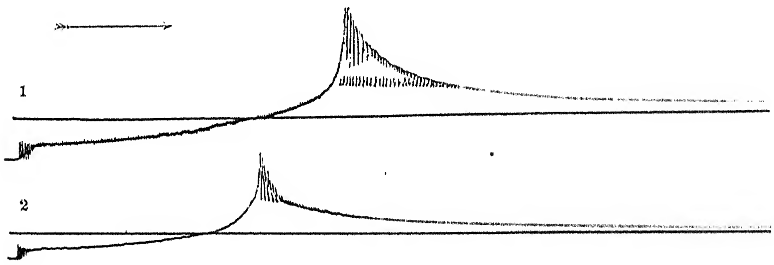


FIG. 10.

The curves shown in Fig. 9 must be accepted as merely illustrations of the form the current curves might assume under certain conditions. Since they are obtained on the assumption that the current I_0 dies away uniformly, it is scarcely to be expected that the photographs obtained of the current through B during the "break" would agree in detail with the curve drawn above. Since in the present investigations the resistance of the branch B was 300 times that of C, no attempt could be made to draw the curves in Fig. 9 to represent the magnitude of the currents in B and C during this investigation without destroying the clearness of the figure.

It has been difficult to obtain photographs of I_B which are distinct enough to reproduce for publication. For the current curve is irregular, and since the needle possesses some inertia, the very sudden changes in the current produce vibrations which

are difficult to photograph. It has therefore been necessary, in order to reproduce the faintest of these curves, to print from positives on bromide paper and sketch in the outline obtained.

Fig. 10 (1) (2) show current curves for the branch B, reproduced in this way. The curves are traced from right to left. The straight part of each curve at the beginning is the steady deflection

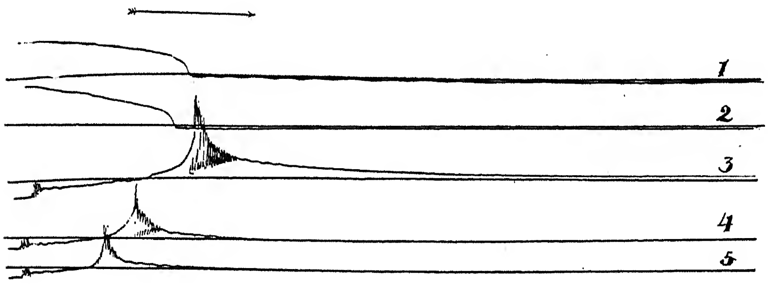


Fig. 11.

of the needle from zero, representing the current flowing before the break. This deflection was to the left, and hence appears below the zero line, as viewed in Fig. 10. The point where the break begins is clearly marked by a sudden deflection toward the zero line. From that point the curve dies away to the zero line, crosses it, rises to a maximum on the other side, and from there

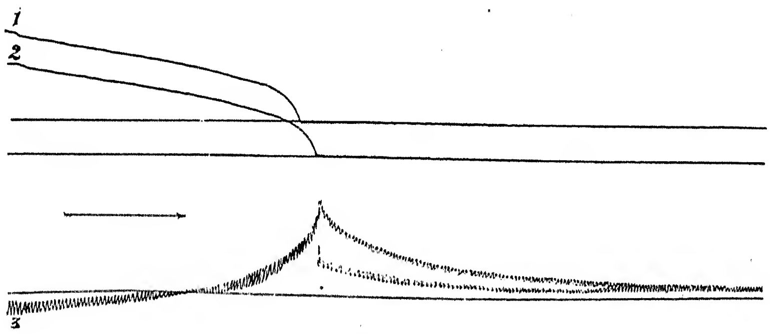


Fig. 12.

falls to zero by a curve logarithmic in form. The marked vibration of the needle at or near its point of maximum deflection is striking, and shows that at that instant the current suddenly changes in magnitude.

From the curves obtained in Fig. 9, we see that the current curve for B undergoes such a change at the point P, where we

have supposed the spark to end, and we therefore might conclude that these violent vibrations of the needle in Fig. 10 would mark the ending of the spark in the branch A. This conclusion is supported by Fig. 12, in which (3) represents another current curve for the branch B, while (1) and (2) show current curves for A and C, which were obtained by shunting the galvanometer across a non-inductive resistance of 3.385 ohms, first in the branch A, and then in the branch C, the branch B being kept complete and of the same resistance as when the galvanometer was in it. Since these photographs were obtained consecutively and not simultaneously, and since it is doubtful if two sparks can be obtained of exactly the came character, these curves can not be rigorously compared. Considering the variable nature of a spark, the close agreement between the time at which the spark ends in A and C, as shown in (1) and (2), and the time at which the

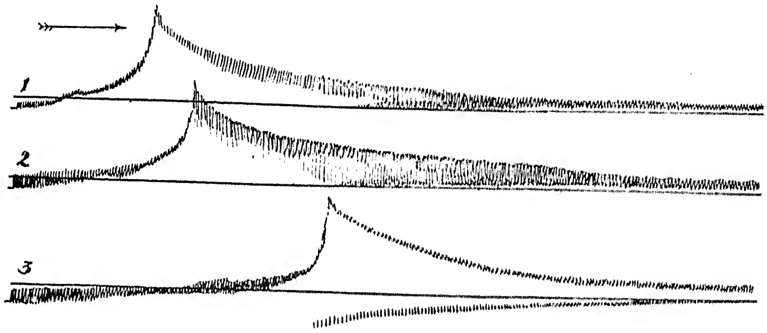


FIG. 13.

marked disturbance of the needle occurs in B, as shown in (3), justify the above conclusion. Fig. 11 (1) (2) (3) show another such group in which the agreement is not so exact.

In comparing (2) and (3) Fig. 12, the current curves for C and B, it might appear that after the break I_c is zero, while I_B clearly does not reach zero for an appreciable time. But we have seen that after the break, I_c and I_B have the same value! The explanation of this apparent inconsistency is found in the fact that the scale to which I_B is traced is twenty times as great as that to which I_c is traced, and hence a current which is represented by a considerable ordinate in the one case, is scarcely noticeable in the other.

Figs. 10 and 11 (3) (4) (5) illustrate the effect on the current through B, of varying I_0, the current through A at the instant of

breaking the circuit, other conditions of the circuit being kept as constant as possible. In Fig. 11 (5), I_0 is .75 amperes; in (4), 1.5 amperes; in (3), 2.25 amperes, and in Fig. 10 (2) and (1), I_0 is 3.5 and 5.2 amperes, respectively. The curves show us that increasing the current I_0, increases the duration of the spark, and the maximum point to which the current curve I_B rises.

Fig. 13 illustrates the effect on I_B of increasing the strength of the spring in the circuit-breaker. We have already seen in Figs.

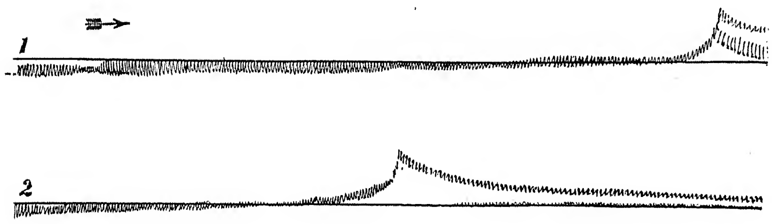


FIG. 14.

6 and 7 that increase of the strength of the spring in the circuit-breaker decreased the duration of the spark. The curves (1) (2) (3), taken with decreasing strength of the spring, further illustrate this. No marked effect on the height to which I_B rises can be noticed. As nearly as the curves can be measured, (2) and (3) rise to the same height, while (1) is slightly less than either.

Figs. 14 and 15 illustrate the effect on I_B of change in the E. M. F. in the branch A, the current I_0, and speed of breaking being kept constant. In Fig. 14 (1) E is 300 volts, in (2), 170

FIG. 15.

volts, and in Fig. 15, 58 volts. In the first case, the spark lasts .115 second; in the second, .066 second, and in the third, .018 second. The height to which I_B rises decreases as E increases, being .4, 1.25 and 1.4 cms. above the zero line in the three cases, respectively.

Since the resistance of the branch B was very large compared with that of C, the one being 1570 ohms and the other 5.815 ohms, and since its self-induction was negligible, the current

through the galvanometer B may be taken as a measure of the difference of potential between the points F and G (Fig. 4). Since this potential difference after a short time is largely due to the induced E. M. F. in the branch C, and, after the break, wholly due to it, the current curve for B may be taken as indicating the

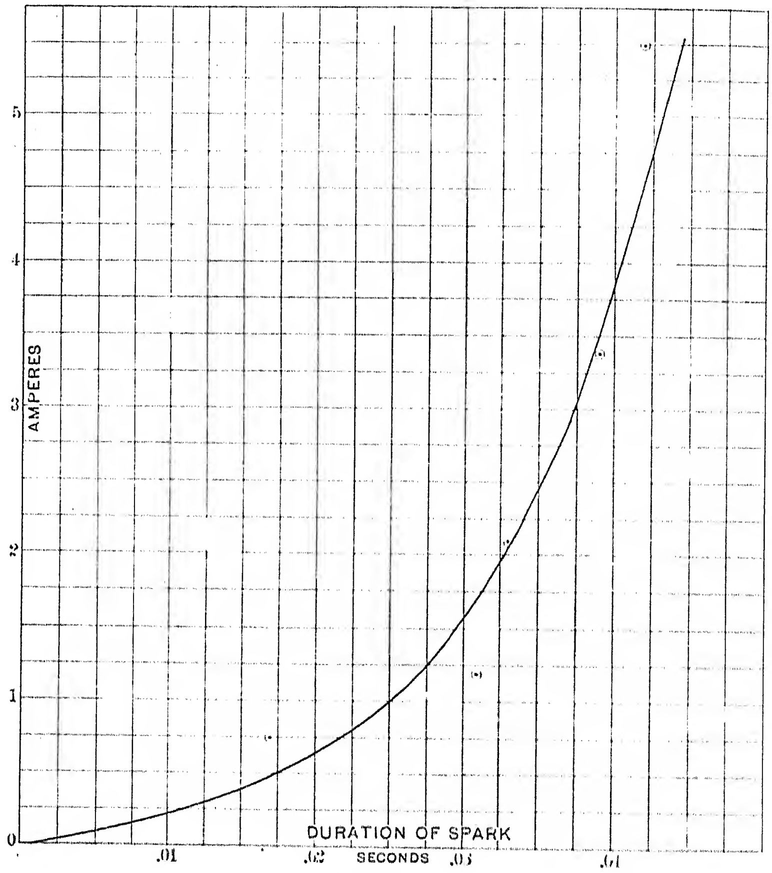


FIG. 16.

variation of this induced E. M. F. during the break, and as approximately representing it in magnitude.

From the curves shown above, we gather that the arc formed by a break is exceedingly variable in character and duration, and that the E. M. F.'s induced by the break vary with the arc formed.

We note that (*a*) the duration of the arc depends upon:

I. The current flowing at the instant of break. The variation

of the duration of the arc with the current, as calculated from Figs. 10 and 11 (Table I.), is shown graphically in Fig. 16.

TABLE I.

No.	I_0 Amperes.	E Volts.	Terminals of Circuit Breaker.	Duration of Spark, Seconds.	Maximum P. D. at points F & G Volts.	Maximum Current Flowing Through B Amperes.
Fig. 6 (1).......	5.2	170	Copper	.065	—	—
" (2).	5 2	170	Brass	.075	—	—
" (3).... ..	5.2	170	"	.068	—	—
Fig. 7 (1).......	1 5	170	Brass	.066	—	—
" (2).......	5.2	170	"	—	—	—
" (3).	5.2	170	Copper	.115	—	—
Fig. 8 (1).......	2 15	169	Zinc	.042	—	—
" (2).......	2.15	169	Zinc	.046	—	—
Fig. 10 (1).......	5.5	125	Brass	.047	62.0	.042
" (2).... ..	3.4	125	"	.061	57.0	.038
Fig. 12 (1).......	4.2	125	Brass	.055	—	—
" (2)..... .	4.2	125	"	.057	—	—
" (3).	4.2	125	"	.057	40.5	.027
Fig. 11 (1)..	2.1	125	Brass	—	—	—
" (2)... ...	2.1	125	"	—	—	—
" (3).	2.1	125	"	.031	49.5	.033
" (4).......	1 2	125	"	.026	46.5	.031
" (5).......	.75	125	"	.015	25.5	.017
Fig. 13 (1),.. .	2.75	125	Brass	.024	30.0	.020
" (2).......	2.75	125	"	.034	45.0	.030
" (3).......	2.75	125	"	.058	46 5	.031
Fig. 14 (1).......	2.25	300	Brass	.115	15 0	.010
" (2).... .	2.25	170	"	.066	45.0	.030
Fig. 15 (1).......	2.25	58.5	Brass	.018	51.0	.034

II. The E. M. F. of the battery in the branch A. An increase of the battery E. M. F., other things being equal, always increases the duration of the arc.

III. The speed at which the terminals of the circuit-breaker are separated. Increase in the speed at which the terminals of the circuit-breaker are separated hastens the destruction of the arc. Figs. 13, 6 and 7 illustrate this point.

IV. The metals of which the terminals are composed. We have seen in Figs. 6 and 7, that a break by copper terminals is of shorter duration than a break from brass. A break with zinc terminals does not seem to differ greatly in duration from a similar break with brass.

(*b*) The character of the arc formed varies, of necessity, with any condition that affects the duration of the arc. In particular, however, zinc terminals seem to produce a very irregular arc. In Fig. 8, the irregularities in the current curves are peculiar to zinc breaks. The curve for I_B with zinc terminals, not shown in this paper, possesses similar irregularities.

(*c*) The E. M. F.'s induced by the break, as indicated by the induced current flowing through B, vary also, of necessity, with any

condition that affects the duration of the spark. The maximum induced E. M. F. varies with :

(1) The current flowing at the instant of break. From Figs. 10 and 11, as tabulated in Table I., Fig. 17 is plotted, showing the variation of maximum induced E. M. F. with the current.

(2) The E. M. F. of the battery. The variation is shown

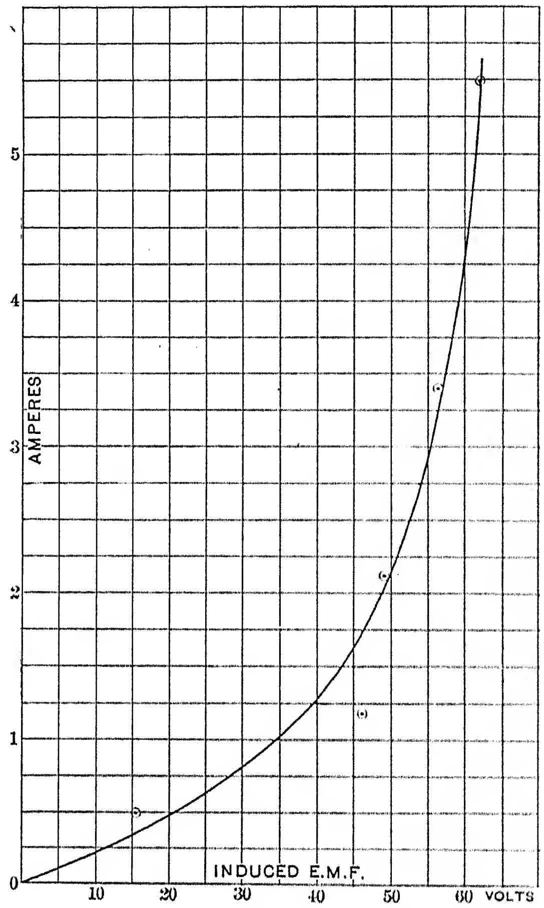


FIG. 17.

in Table I. An increase of E. M. F. prolongs the duration of the spark and prevents a high induced E. M. F.

(3) The speed of breaking. Owing to the vibrations of the needle, the height to which the curve actually rises is difficult to determine. There seems to be very little difference, however, between these curves in this respect. Other experiments, how-

ever, described below, show that the maximum induced E. M. F. increases with the speed of breaking.

The apparatus described above was constructed with the aid of Mr. W. J. Lester, and, with his help, experiments were made to determine the E. M. F. induced in the field of a motor when the current flowing through it is broken. The motor used was a Thomson-Houston, No. 3322, Class 30 R. 500 volts. The field coils in parallel had a resistance of 2.05 ohms and a self-induction

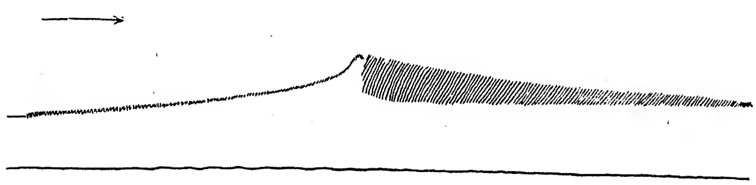


FIG. 18.

of .15 henry. The arrangement of apparatus was as before (Fig. 4), C being, now, the field of the motor disconnected from the armature, while the source of E. M. F. in A was 100-volt mains.

In order to hasten the destruction of the arc, in some of the experiments an electro-magnet was placed so that the arc formed in a magnetic field. Under such circumstances, the curves obtained were very irregular and the vibrations of the needle became so great that it was found necessary to drop the plate more slowly,

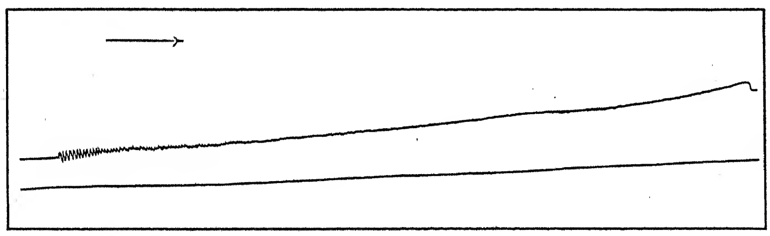


FIG. 19

in order to get greater effect on the plate. By means of a pulley fixed at the top of the slide, over which passed a cord to which was attached the plate holder and a counter-weight, any desired speed could be obtained.

The electro-magnet, used to blow out the arcs, consisted of 100 turns of No. 18 B. and S. copper wire, wound on wrought-iron pole-pieces about 2.5 cms. in diameter, on which were mounted tapering pole-tips. This magnet had been previously calibrated

in the laboratory, but since it was inconvenient to place the terminals of the circuit-breaker directly between pole-tips, the strength of the magnetic field could only be estimated very approximately. During the experiments, the strength of the field varied probably from 1,000 to 3,000 lines per sq. cm.

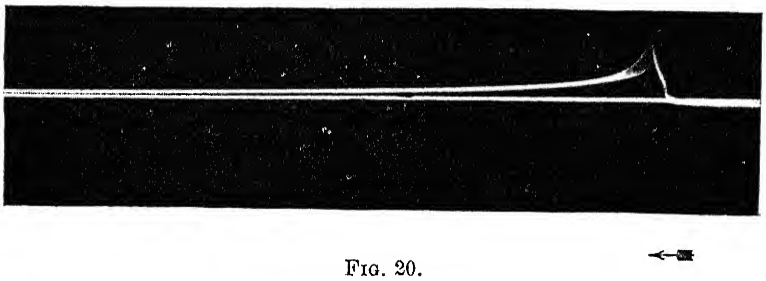


Fig. 20.

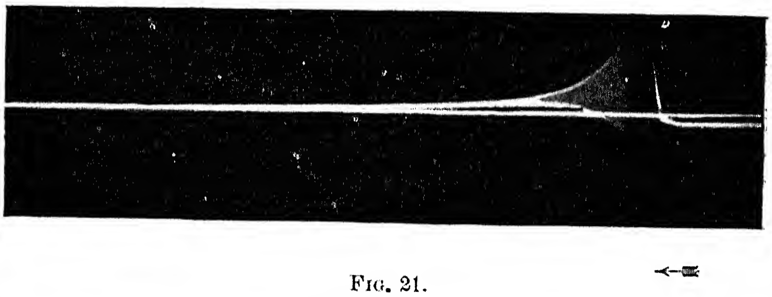


Fig. 21.

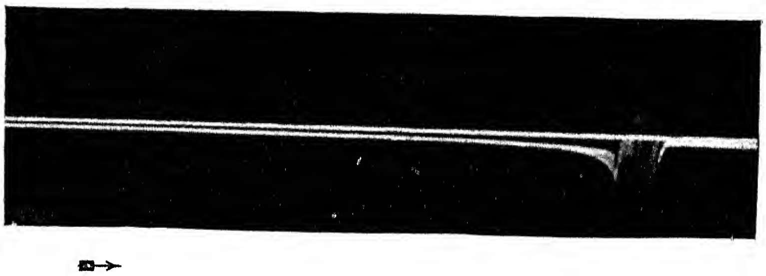


Fig. 22.

Fig. 18 is one of the curves obtained. An interesting point to be noticed is that the curve passes its maximum point before the spark ends, differing in this respect with the curves above. Other curves were obtained, showing this same feature. I_0 is 11 amperes, the duration of the spark .05 seconds, and the maximum induced E. M. F. 365 volts.

Fig. 19 shows a curve obtained when I_0 is 42 amperes. The spark lasts much longer. No marked disturbance can be detected on this curve to mark where this spark ends, but only a slight vibration after the drop from the highest point of the curve. The duration of the spark is probably .14 second, and the maximum induced E. M. F. 265 volts, which is less than in the preceding case when I_0 was only 11 amperes.

Fig. 20 shows the form of curve when the plate is dropped slowly and the arc blown out by a magnet. I_0 is 12 amperes, and the duration of the spark .03 second approximately. The maximum induced E. M. F. is 500 volts.

Fig. 21 is a similar curve obtained when I_0 is 11.5 amperes and the strength of the magnetic field increased. The short duration of the spark and the violent ending are noticeable.

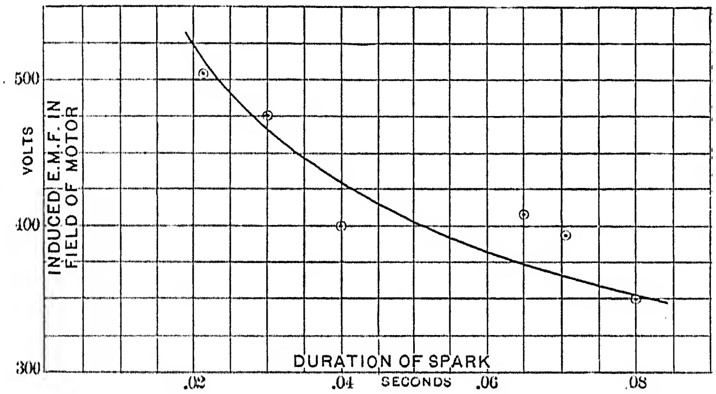


FIG. 23.

Fig. 22 illustrates a very irregular break. A number of curves were obtained of this form when the resistance in series with the voltmeter was 33,000 ohms. Further investigation is required to determine the cause.

In a thesis by W. J. Lester and the writer (Sibley College Library, Cornell University), descriptions and photographs of all the curves obtained may be found. Table II. shows data of some of the curves, while Fig. 23 is a curve, from the thesis, showing the variation of maximum induced E. M. F., with the duration of the spark, I_0 being constant and equal to 20 amperes.

TABLE II.

No.	I in Field. Amperes.	Duration Spark.	Induced E. M. F. Volts.
1	20.5	.08	430
2	21.2	.07	395
3	11	.05	365
4	20	.08	355
5	32	.08	290
6	42	.14	265
7	20	.02	475
8	20	.02	505
9	32	.03	530
10	32	.02	585
11	11.5	.04	370
12	12	.05	490
13	13	.05	295
14	13	.02	340
15	29	.02	710

The results presented in this paper must be accepted as illustrative of the method of experimentation adopted, and as outlining problems, which the writer hopes to be able to investigate further. Besides a further investigation of the points already discussed, other equally interesting problems present themselves for solution, such as the effect of breaking from the surface of liquids, the effect of the form of terminals used, and the variation of the current curve in each branch of this circuit, as the relation between the resistances of the branches is varied.

Discussion.

Prof. Anthony:—I do not rise to discuss the paper at all, but simply to express my gratification that the subject has been brought before the Institute. About a year ago, I saw the apparatus at Cornell University. It all depends on the extreme delicacy of the little magnet, having such an extremely high period of vibration of its own, that the vibration rate is entirely removed from the results that are obtained. In other words, it does not in any way affect the results that are obtained. Before I saw this apparatus, however, Prof. Brackett, of Princeton, had shown me a somewat similar arrangement of his own, which he had been at work upon without knowing anything about the work being done in the same direction at Cornell. I speak of this simply to call attention to a very interesting investigation which he was making, which I believe has also been carried out with this apparatus. By the use of two mirrors, one placed in a coil across an alternating circuit, and the other in a coil included in the alternating circuit, and throwing the reflected light to the same point when the mirrors were in equilibrium, he was able to obtain the relation between the alternating electromotive force

and the current of an ordinary alternating machine. He had a number of results which showed that very nicely under different conditions. One mirror, of course, would vibrate in phase with the current; the other in phase with the electromotive force.

MR. A. J. WURTS:—As I understand the speaker, the fluctuations with brass electrodes were quite marked. I should like to ask what the composition of the brass was?

DR. NICHOLS:—It was commercial brass. I do not know the composition. It was probably rolled brass. I should say it was a brass and not a bronze.

[At this point the President called Vice-President Steinmetz to the Chair.]

THE CHAIRMAN:—Gentlemen, after this interesting paper, I think a vote of thanks should be given to Mr. McKittrick and Dr. Nichols.

[On motion, a vote of thanks was tendered to the gentlemen named.]

MR. HOWELL:—I also think the INSTITUTE should make some expression of its appreciation of the labors of the Committee, in making the report on Standards of Light, which was presented to us this morning. I make a motion to that effect.

[The motion prevailed.]

THE CHAIRMAN:—In the name of the INSTITUTE, I thank the gentlemen for the very valuable reports submitted, and I hope the Committee will continue their work on Standards of Light, and bring it to as satisfactory a conclusion.

DR. NICHOLS:—On behalf of the Committee, I should like to say that the work has only been begun, and I think it would be quite proper for the INSTITUTE to withhold its thanks until it sees whether the Committee succeeds in evolving anything from its labors, or not.

THE CHAIRMAN:—While the work of the Committee has not been completed, and it has not been possible yet to make a definite recommendation of a standard of light, sufficiently valuable work has been done and embodied in the report under discussion, with regard to the features of the various proposed standards, the sources of error to be guarded against, and the relative value of the different proposed systems, so that I think the INSTITUTE is fully justified in agreeing upon a vote of thanks to the Committee.

MR. HOWELL:—We ought to thank the Committee, at least, for the expression of inaccuracy regarding our present standard. I read this week in a paper published at one of the leading technical schools of this country, by one of their graduates, who is a gas engineer, and the subject was Practical Photometry, or Commercial Photometry. It described the photometric apparatus used in gas works, and it concluded by saying that photometric observations were reliable within one per cent., which is very much in contrast with what Dr. Nichols told us this morning.

A paper presented at the 13th General Meeting of the American Institute of Electrical Engineers, New York, May 20th, 1896. Vice-resident Steinmetz in the Chair.

THE RECONSTRUCTION OF THE PLANT OF THE CHICAGO BOARD OF TRADE.

BY BION J. ARNOLD.

When the writer agreed some four weeks ago to prepare a paper on the reconstruction of the plant in question, he hoped to be able to present to the INSTITUTE to-day some records of its operation, or data which would be of value, but owing to the delay in the arrival of certain parts of the plant, the question of operating it a sufficient length of time to secure such data proved impracticable, and not wishing to appear before this body with any data the completion of which did not extend over a sufficient length of time to make it reliable, he must confine himself to-day practically to a description of the plant, with the expectation of presenting to this INSTITUTE at a later date the records of its operation, in a form which he hopes will be of some permanent value. The plant involves a number of departures from the standard lines of office building engineering, and if the results of its operation are as successful as it now seems they will be after running a short period, the annual expenses of the operation of the plant will be reduced from $25,000 to $15,000, consequently its operation will be watched closely by those who have the matter immediately in charge.

The writer believes that all the energy required to produce motion and light in an office building should be developed in one set of steam engine cylinders and on one generator, having, of course, one set of cylinders and their generator in reserve. This set of cylinders, together with the working parts of the engine and its generator connected directly to it, constitute the unit which produces the energy of the plant, and this unit should be made to

work at its maximum economical load throughout its entire period of operation, while the energy from the unit should all be utilized during its running time. Having this idea constantly in view, the designer of this plant has planned to follow it out as closely as possible.

The old plant consisted originally of six horizontal tubular boilers operating at a steam pressure of about 75 to 80 pounds, and driving five small steam engines distributed in different

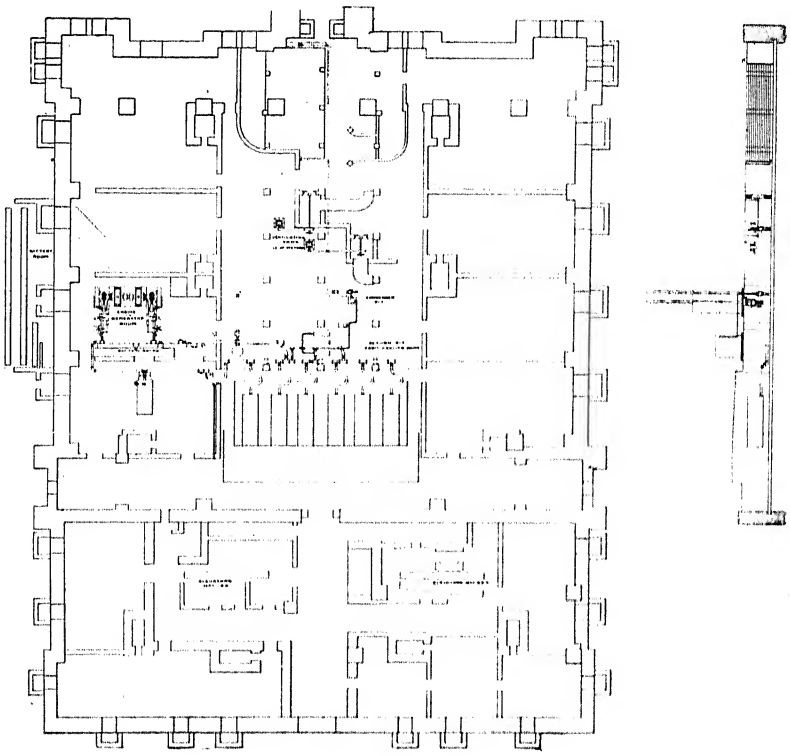


FIG. 1.

parts of the building, together with the necessary steam pumps, air compressors, etc., used for feeding the boilers, and operating the hydraulic elevators. Two of these engines were belted to a line shaft and drove several small incandescent and arc dynamos for lighting the building. The three other engines were used for driving the ventilating fans, three of which were located in the attic and two in the basement, and were driven by the engine by means of rope-drives with sheaves and ropes running at various

angles throughout the building. The elevators were driven by two horizontal compound direct-acting pumps, which consumed from 80 to 100 pounds of water per horse-power of energy delivered to the elevator cars. It became apparent after an examination of the plant that if the steam pressure could be increased to 125 pounds per square inch, so as to get the advantage of drier steam, and all the pumps in the plant which were consuming steam full stroke eliminated, and the energy of the plant produced by a compound condensing engine running at an economical load, that a large reduction in the operating expenses of the plant could be effected. The old boilers having been in use for about nine years, were almost ready to be condemned by the boiler inspectors, consequently it was an easy matter to decide upon replacing the boilers with heavier ones which would work at the above pressure. The adoption of a compound condensing engine to work under this increased steam pressure was a natural sequence which enabled the energy to be produced with the least possible coal consumption. After quite an extended investigation it was decided to supplant the hydraulic elevators with horizontal screw multiple sheave elevators, as the investigation showed that these machines could be operated for considerable less money per car mile than the hydraulic machines, under the conditions which existed in this plant. The operation of these elevators in practice has fully proven the correctness of this position. They have been in operation about four months now, on an average consumption of 4¼ kilowatt-hours per car mile, and as the duty required of them is exceptionally heavy, and the cars very large, this showing is very satisfactory.

The general plan of the plant is as follows:

Referring to Fig. 1, which represents the basement of the Chicago Board of Trade, the relative location of the different machines which enter into the plant can be determined. It will be seen that the installation consists of the following:—Five 66x 16 ft. horizontal tubular boilers, designed to carry 125 pounds pressure per square inch; two 150 H. P. horizontal compound condensing engines running at 275 revolutions per minute, each directly connected to a 75 K. W. direct current generator, under a special system hereinafter described, which permits of either or both generators being driven from either engine. Four horizontal 30 H. P. multiple sheave elevators; six 10 H. P. electric motors, five of which operate ventilating fans, and one the machinery in

the machine shop, 50-2000 c. p. constant potential arc lamps and 600-16 c. p. incandescent or glow lamps. There are also sixty-five 1600 ampere-hour storage cells, and the necessary switch-board and connections for the handling of the above machinery, all of which is more distinctly described hereafter. One of the compound steam pumps has been retained in order to keep the tank on the roof of the building supplied with water for use in the wash-basins, closets, etc. It may prove advantageous in the future to substitute for this an electrically driven pump, but

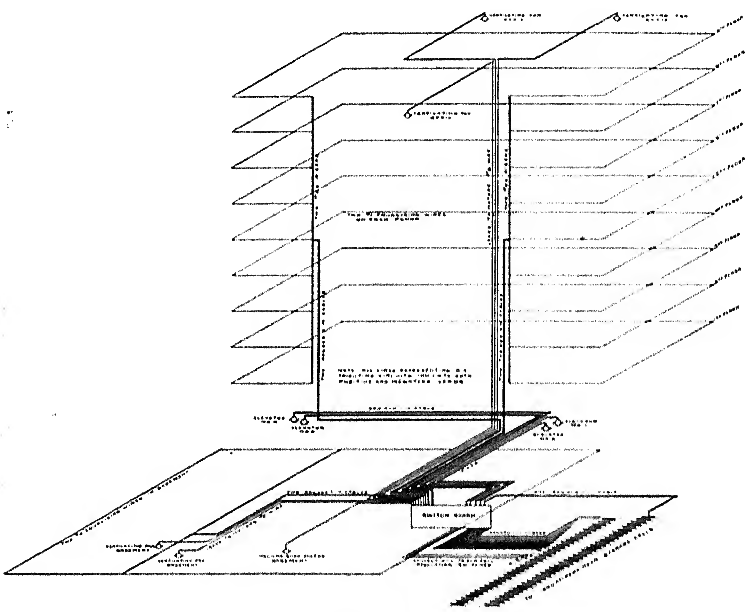


Fig. 2.

it has been thought best not to discard the old one at present, inasmuch as it is already on hand.

Fig. 2 represents diagrammatically the main leads or copper conductors of the building, used for the operation of the above described electrical machinery. The general plan is clearly shown by the diagram, and this system was adopted owing to the fact that the building was so constructed that two main flues or passageways extending from the basement to the attic, in opposite portions of the building, and near its centres, were available for carrying the main risers. It was decided to place in these passageways vertical risers of sufficient carrying capacity to conduct

the total amount of energy required for the nine floors, and to join the floor leads or secondary mains onto each of these risers at their ends. With this arrangement it was possible to feed the secondary mains at two independent points, thus equalizing the pressure to better advantage and making the plant more reliable in operation. From the centre of these vertical risers, or at about the fifth floor, the main leads drop down each shaft, thence extend horizontally on the ceiling of the basement to the switchboard. Each of these mains leading to the centre of distribution in each shaft is of sufficient carrying capacity to carry the total load of the building, exclusive of the basement, consequently if a fuse blows on either one of the mains, or if by any accident one of the mains should become broken or opened, the other main will carry the necessary current and keep the lights operating in the building. This precaution was taken owing to the fact that business matters of great importance are transacted in this building, and the loss of a few minutes at certain times would be disastrous to the men who operate in the building, consequently every precaution for safety and reliability had to be taken.

The basement circuit is independent of the above described mains, and consists of positive and negative leads, surrounding the entire basement of the building, from which are tapped the arc and incandescent lights used in the basement. The motor circuits are closely indicated in the diagram, and are independent of the lighting circuits, and are so planned that the starting rheostats of the motors are connected on the switchboard under the control of the engineer, thus making it possible for the engineer to start or stop any motor in the building without leaving the switchboard. The elevator circuits are also independent of the lighting and motor circuits, and it is possible to throw the current on or off any elevator at the switchboard. The batteries are located in a room adjoining the main engine room, and connected with the switchboard, as shown on the diagram.

The losses in wiring circuits of the building are computed as follows: Between generator and switchboard, one-half of one per cent.; between switchboard and centre of distribution, two and a half per cent.; from centre of distribution to secondary mains, one per cent.; between secondary mains and lights, one per cent.—making the total loss between generator terminals and lights five per cent.

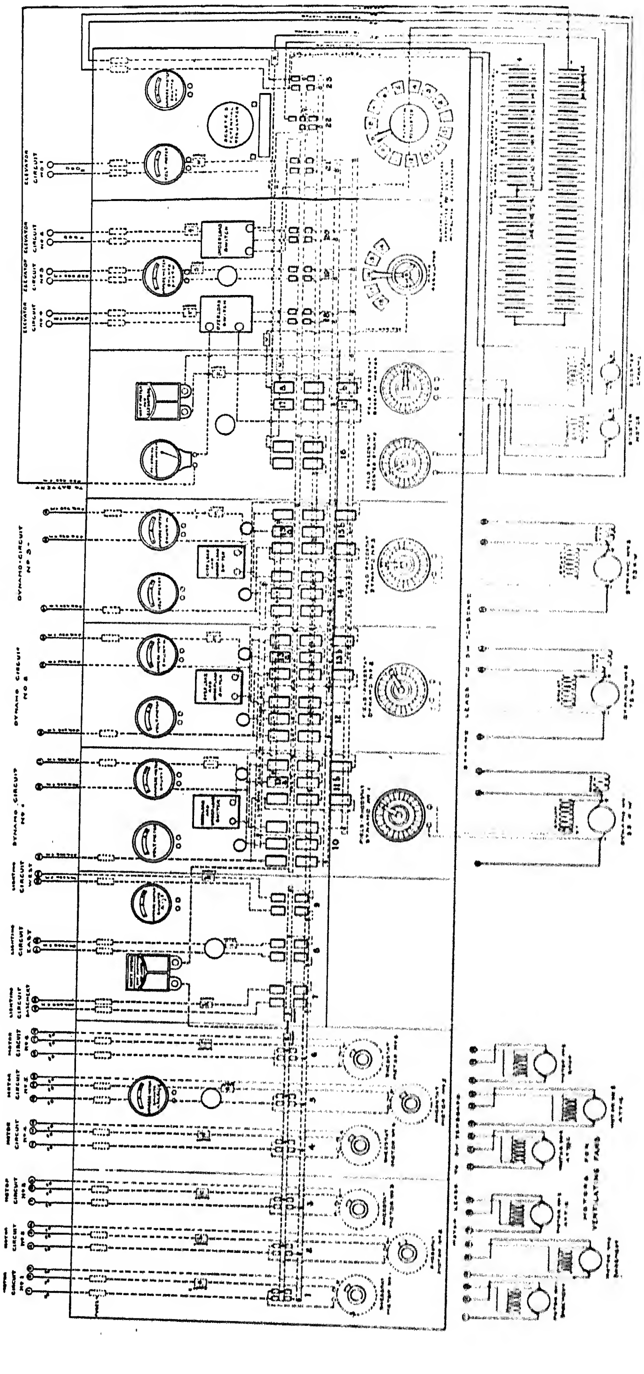


Fig. 3.

Fig. 3 represents largely diagrammatically the switchboard of the plant. From this it will be noticed that the plant is designed for three generators, but two of which are now in operation, as they are sufficient for the work. The lighting 'bus bars carry the main lighting circuits, controlled by switches 7, 8 and 9, and also the motor circuits controlled by switches 1, 2, 3, 4, 5 and 6. In the main lighting 'bus bar is placed a recording wattmeter, which registers all the energy delivered from the generator to the light and motor portions of the plant. In the elevator 'bus bars is placed another recording wattmeter, which registers all the energy delivered to the elevators from the lighting 'bus bars. With this arrangement and the means available in the plant for measuring the amount of coal and water consumed, the operators are enabled to keep accurate daily records of the cost per kilowatt of the energy produced and delivered by the plant. The diagram of the switchboard clearly shows the windings of the different generators and motors, and need not be further explained.

The generators are connected to the board by means of two switches, one of which is a triple-pole double-throw switch, and is arranged so that when thrown into the upper position it connects the generator to the elevator 'bus bar, using the compound winding of the generators. This arrangement permits the generator to operate the elevators and take care of the variable load without the use of the batteries. When the same switch, for instance No. 13 or 15 on the accompanying diagram, is thrown in its lower portion, the circuits are so arranged that the compound or series winding of the generator is cut out, and the equalizer connection from the generator becomes the positive connection, and the generator is connected to the elevator 'bus bars shunt wound, thus eliminating its series winding. With this arrangement, the generators and the batteries are run in multiple on the elevator load.

Referring now to the right-hand part of the board which is the storage battery portion proper, the operation is as follows: Under normal conditions, any elevator is operated shunt wound in parallel with the batteries by closing its corresponding switch, as, for instance, switch 15 for dynamo No. 3, downward the 15 *b* position. By closing switch 17 to 17 *a* position, the current passes from the positive elevator 'bus bar to the hand regulating cell switch, and out to cell No. 16, when the regulator is placed as shown

on the diagram. From cell 16 the current passes through the series of cells coming out at the negative end, thence through the proper conductor to the recording ammeter, thence to the overload switch and down to the negative terminal of switch No. 17 *a*. It will now be seen that whatever number of batteries are being operated in this series, are in parallel with the generator. The hand regulator is placed in the position indicated to enable the operator to cut in a sufficient number of cells in parallel with the generator, so that the cells will be constantly charging during the time of operation of the elevators, except at the temporary moments of overload caused by an excessive demand for current by the elevators. When this pull from the elevator occurs, the batteries respond and take the surplus load from the generator. With this arrangement, about fifty of the cells will be kept constantly charged, and in parallel with the elevators. In case the amount of current entering the cells is excessive between the intervals of heavy load on the elevators, the hand regulator is adjusted so as to cut in one or more cells until the current becomes reduced to the proper amount for the batteries; and, on the other hand, if the amount entering the cells is not enough to keep them properly charged, the hand regulator is adjusted so as to cut out a number of cells until the proper amount of current is reached. In the meantime, if the 15 end cells require charging, they are charged by means of the independent booster or motor generator in the manner hereinafter described. All the batteries can be operated in parallel with the generators when running shunt wound, in the same manner as they are used in conjunction with the elevator 'bus bars, by closing switch 17 downward to position 17 *b*. The connections for same can be easily followed on the diagram.

In order to operate the batteries in parallel with both the lighting and the elevator 'bus bars at the same time and maintain a practically constant voltage on the lighting 'bus bars, it is necessary to operate the generators shunt-wound instead of compound-wound. As before shown, the switches are so arranged that the compound winding of the generators can be utilized when the batteries are not in service, and the shunt winding used when the batteries are being run in parallel with the elevators and lights. Therefore, to maintain a practically constant voltage on the lighting 'bus bars and to run the elevators in parallel with the batteries at the same time, the operation is as follows:—Switch 17 *a*

is closed upward and operates as previously described in conjunction with the elevators and batteries. Switches 10, 12 and 14 are open to prevent the generators operating compound-wound on the lighting 'bus bars. Switch 16 is closed and the current passes as follows:—From the positive elevator 'bus bar through the switch 17*a* to the centre of the hand regulating switch, thence through the connection from the hand regulating switch to the corresponding cell (shown on the drawing as cell 16) thence through the series of 49 cells, out through the negative end and its corresponding connection, through the recording ammeter and overload switch, to the negative side of switch 17 *a*, thence to the negative elevator 'bus bar. It should be borne in mind that the lighting 'bus bars and the elevator 'bus bars are operating at different potentials, and that the potential on the elevator 'bus bars is changing slightly with the change of load, caused by variable load on the elevators. When the demand for current from the cells is great, as for instance when the elevator load is severe, the E. M. F. of the number of cells in parallel with the elevators necessarily drops, consequently the E. M. F. of the number of cells in parallel with the lighting bars, which includes all the cells working with the elevators, and a few of the regulating cells, would correspondingly drop, and some means must be provided to hold up the voltage of the lighting 'bus burs. This is accomplished by means of the automatic regulator which is controlled by a solenoid switch, shown in the upper right hand portion of the diagram. This solenoid switch operates by means of suitable connections to the lighting 'bus bars, in such a manner as to make and break the circuit through mercury cups as follows:—When the voltage of the lighting 'bus bars is reduced slightly, one side of the field of a right and left hand motor is thrown in, which causes the arm of the automatic regulator to move clock-wise, thus cutting in more cells in parallel with the lighting 'bus bars and holding the E. M. F. up. When the E. M. F. of the lighting 'bus bars reaches its proper voltage and tends to exceed it, the solenoid acts in the opposite direction and cuts in the left hand field of the automatic regulator motor, and brings the left hand field into service, causing the automatic regulator to rotate contra-clock-wise, thus cutting out cells until the voltage reaches the desired point. By this means a constant E. M. F. is maintained on the lighting 'bus bars, and the cells are worked in parallel with both the lighting and elevator 'bus bars.

It will be evident that the 15 end cells, or at any rate a portion of them when working in this manner, will become discharged through the day's run. After the batteries are cut off from the light, these end cells are re-charged as follows: Switch No. 23 is closed, thus throwing in the booster motor. As soon as the booster dynamo is brought up to speed, the underload switch is closed, and switch No. 22 closed upward, the operation then being as follows: From the positive brush of the generator end of the booster, which is producing energy at a low voltage, the current passes through the proper conductor to the centre of the automatic regulator; thence out through the regulator arm to segment No. 1, and through its proper connection to cell No. 1; thence through the first 15 cells to the wire leading off between the fifteenth and sixteenth cell. From this point it passes along the conductor, through switch No. 23 to the underload switch; and from the underload switch back to the negative side of the booster generator. By means of a field rheostat, located on the switchboard, the E. M. F. of the booster generator is regulated and adjusted to correspond to the number of end cells which are being charged.

To charge all the cells in series from one of the main generators, it is necessary to increase the E. M. F. a sufficient amount to overcome the total voltage of the cells. This is done by placing the generator portion of the booster or motor-generator in series with the main generator as follows: Switch No. 23 is closed, thus throwing in the booster motor and at the same time energizing the fields of the booster dynamo. The booster motor is now started by means of its rheostat and brought up to speed. Switch No. 22 is closed downward, the path of the current then being as follows: From positive lighting 'bus bar through switch 22, to the underload switch, thence to the negative brush of the booster dynamo, out through the positive brush to centre of the automatic regulator, through this arm to segment No. 1, and thence through suitable connection to cell No. 1; through the entire series of cells to the negative lead, thence through the recording ammeter and overload switch; back through switch 22 to the negative lighting 'bus bar. The operations for manipulating the entire plant being then as follows:—

To charge all the cells in series with the main generator by means of the booster: Open switches 16 and 17 and close switches 23 and 22 down.

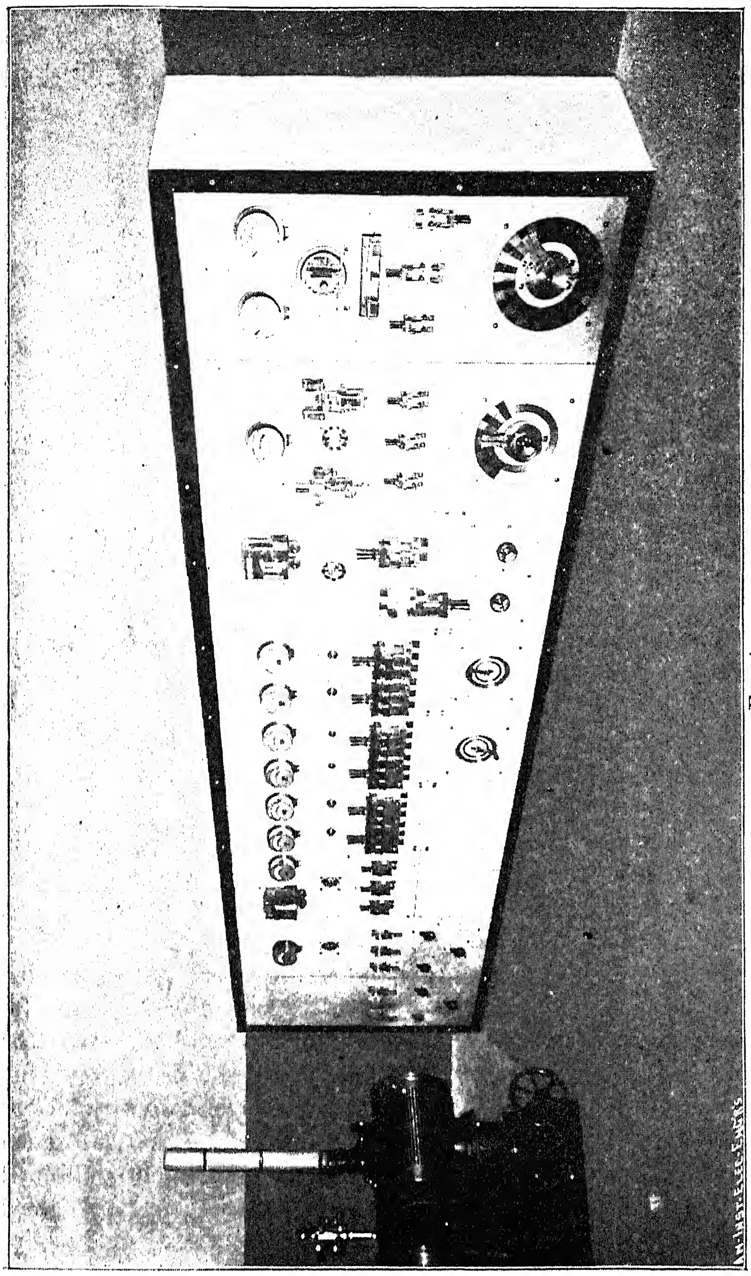


FIG. 4.

To charge all the batteries, except the end cells, at the same time operate light and elevators from one dynamo. Place hand regulator on segment 16, connect whatever dynamo is in use to elevator 'bus bar by means of its elevator switch, either 11, 13 or 15, in the *b* position. Disconnect dynamo from lighting 'bus bars by opening its corresponding switch, either 10, 12 or 14, and see that switches 22 and 23 are open, and that switches 16 and 17 are closed, the latter in the *a* position.

To charge end cells with booster dynamo:—Place automatic regulator on segment No. 1, see that switch No. 16 is open, and close switches 23 and 22 up.

To place elevators, batteries and one of the generators in parallel, close the corresponding generator switch to the *b* position, and close switch 17 to the *a* position, if desired to run through the hand regulator, and to the *b* position if desired to operate through the automatic, but if placed in the *b* position, see that switch 16 is open.

To discharge the batteries with the generators shut down, operate as follows: On lights, open switches No. 22 and 23, and close switch 16. On elevators, close switch 17 to the *a* position. To operate lights and elevators, see that switches No. 22 and 23 are open, and close switches 16 and 17, the latter to the *b* position.

Fig. 4 shows the elevation of this switchboard, and from it the position of the instruments shown on the diagram can be easily located.

The method of connecting the engines and generators is shown in Fig. 5, and represents a system planned by the writer, which enables a direct coupled plant to be so built as to have the advantage of an independent unit direct coupled plant, when operating under normal conditions, and all the flexibility of a belted plant in case of accident to any particular part of the plant. The cut shows the plan, elevation and section. It will be noticed that at present there are but two generators and two engines; but when extensions are needed, the right hand engine will be disconnected from its present dynamo, and moved to the right a sufficient distance to allow the placing of an engine of double its capacity in the same position it now occupies, and two generators between the middle engine and the right hand engine, thus making a plant with three engine units and four generator units, all four of which are available from the middle engine,

and two of which are available from each of the outside engines. Referring to the sectional view, A *a* represents the ends of the engine shafts which carry disks H *h*. The generators are mounted upon hollow sleeves or quills, which are supported in independent bearings, P P and *p p*. The ends of these quills are enlarged to form flanges corresponding in diameter and thickness to the engine flanges. Between engine shafts A *a* extends an

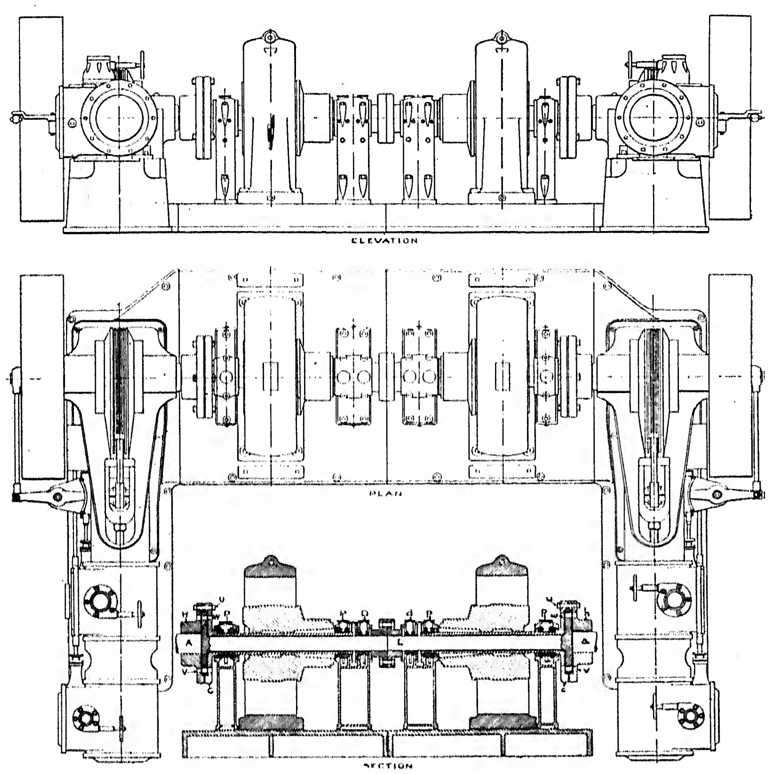


FIG. 5.

auxiliary shaft, E (coupled at its centre, in this case, to permit half of the plant being put in operation before the other half is ready), and carried in independent bearings, D *d*. This shaft, E, carries at each end circular disks, which in this case are forged solid with the shaft. Under normal conditions shaft E does not revolve, but lies passive in bearings D *d*, and the engines drive the generators direct coupled, by inserting the bolts U *u*, three of which, spaced 120° apart, are used in each coupling. It is now evident

that each engine is operating an independent unit direct coupled generator without revolving any more shafting or machinery than is usual in ordinary independent unit direct coupled plants, inasmuch as the quills which surround the auxiliary shaft E do not come in contact with it, but have a clearance of one-quarter of an inch all around it. If from any cause it becomes desirable to drive the left hand generator from the right hand engine, it is done as follows: In the openings *v*, three of which are provided 120° apart, are placed taper bolts which couple the auxiliary shaft E solidly on the engine shaft *a* by means of disk *h*. Taper bolts are also placed in spaces W, which couples shaft E to the quill of the left hand generator. By removing bolts U, which disconnect the left hand generator from the left hand engine, the left hand generator can now be driven from the right hand engine, and the right hand generator continues in operation from the same engine, if desired. By removing *u*, the right hand generator becomes disconnected from the right hand engine, and the left hand generator is being driven from the right hand engine. In the same manner the right hand generator can be driven from the left hand engine, or both generators from either one of the engines. By using compound engines, so designed that by working high pressure steam in the low pressure cylinder, one engine can be made to double its power for a few hours. The plant has thus the total generator capacity available at all times, and allows one engine to be in reserve for repairs, thus doing away with the investment of the third unit, which is usually carried in such plants for safety. The system, when used without the battery auxiliary, instead of being provided with bolted connections as here shown, is provided with magnetic clutches, which permit the generators to be brought up to speed as motors, and thrown in connection with the engines without stopping either engine in use. This makes it possible to connect either generator with either engine without shutting down the plant in any part. With such an arrangement, with the engine and generators equal to the maximum capacity of the plant, and battery auxiliary equal to one-third the capacity of the plant, any office building can be operated in the most economical manner, according to the writer's belief, for when the battery is in operating order, one unit in conjunction with the battery, handles the plant economically and satisfactorily, leaving one engine unit in reserve. In case the battery is out of service, both units can be

operated until the battery is ready for service again, and during the light load period the battery auxiliary takes care of the entire service of the building, without the use of either the engine or generator units.

The engines are operated during the summer compound condensing, by using a cooling pan system and siphon condenser, as shown in section at the right hand of Fig. 1. With this arrangement the condensing water is cooled by means of the 72″ ventilating fan which is in operation during the entire 16-hour run of the plant, and is used for ventilating the building. It was thought advisable to utilize the air from this fan for cooling the water, inasmuch as the energy for driving the fan had to be produced, and utilizing the air from the fan would not entail any additional expenditure of energy upon the plant over that required under ordinary conditions. The condenser is placed near the ceiling of the boiler room and delivers into the standpipe extending downward into a pit about 35 feet. From the bottom of the pit a direct acting deep well pump lifts the water and delivers it to the cooling pans. From the cooling pans the water reaches through suitable piping the cistern shown on Fig. 1. From this cistern the injection pipe from the condenser leads. For this reason the plant will operate about six months of the year compound condensing, and during the balance of the year compound non-condensing, when the steam from the engines will be run into the mains of the building and used for steam heating.

Fig. 6 represents the load diagram of this plant.[1] The parallelogram A, B, C, D, representing the total capacity n kilowatt hours of one of the generator units, assuming that the unit starts at seven o'clock A. M. and operates until eleven o'clock P. M. The line *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, *i*, *j*, *k*, *l*, *m*, represents the load line, or energy required by the building during the same period. That portion of the diagram shaded with lines inclining to the right at an angle of 45° represents the amount of energy passing into the batteries, while the portion of the diagram represented by shade lines inclining to the left at an angle of 45° represents the amount of energy delivered from the batteries. It will be noticed that from 11 P. M. until 7 A. M. the entire load of the plant is operated by the batteries alone. With this arrangement but two shifts of labor are required, one operating from 7 A. M. until 3 P. M., and the other from 3 P. M. until 11 P. M. From 11 P. M.

1. For this diagram see p. 648, vol. xii.

until morning during the winter the watchman acts as fireman for the boilers to maintain the steam heat in the building. This diagram having previously been illustrated in the TRANSACTIONS of the INSTITUTE November 1895, it will not further be described here, except to state that the various lines shown on the diagram represent the loads of the different motors, elevators, etc., and the periods during which they operate. The writer hopes in the future to present to the INSTITUTE a new diagram representing the actual load lines of the plant, compiled from statistics covering its operation for a considerable length of time, and it will then be interesting to compare the actual diagram with this one which was prepared before starting to design the plant.

DISCUSSION.

MR. WOLCOTT:—I would like to ask Mr. Arnold whether it would be cheaper to use some gas now in that building or to use the battery a little more and dispense with the gas. He says some gas is used, as I understand him. I notice that there is a sudden drop in the load at 5:30 P. M. when the gas is turned on, and that allows the surplus current to go into the battery. If that gas were not used, the diagram would be of a considerably different shape.

MR. ARNOLD:—This diagram was prepared in my office before the method of re-constructing the plant was decided upon, and the various lines represent the different motors, elevators, etc., as we supposed they would operate in practice. The point marked "gas turned on" is the time they turn the gas on now,—not what it will be in operation, because we propose to turn the gas off entirely. This diagram was prepared over a year ago. In all probability, within six months from now I will be able to give you a diagram showing what the actual load line of the plant is.

MR. J. W. LIEB, JR.:—I would like to ask Mr. Arnold why it is that in the plant he describes the peak of the load occurs between 11 A. M. and 1 P. M.

MR. ARNOLD:—That is the time when the people connected with the Chicago Board of Trade are most excited. The Board opens at 9.30 o'clock in the morning. At 12.30 they go to their offices and figure up what they have made or lost, consequently the office lights are used most at this time, and some of the motors shown running at this particular period are shut off at a later period, which also accounts for a part of the load.

MR. RIES:—I would like to ask Mr. Arnold to explain a little more fully the precise conditions under which—referring to Fig. 5—one steam engine is cut off and the other one is connected to the intermediate shaft to drive both dynamos. It would seem that this arrangement would not prove very practicable

under full loads, unless the engines are each capable of being worked to double their normal capacity, thereby requiring larger engines than would be employed in ordinary direct-driving installations. But, perhaps, Mr. Arnold may have special reasons for the construction illustrated. What is the object of that?

Mr. Arnold:—That is a condition that sometimes arises. Suppose one engine and the generator attached to the other engine should become disabled, your plant would then be completely shut down if independent units were employed, unless you had a third unit to put in operation. Such a thing is liable to happen, and it is for this emergency that this arrangement is specially adapted, although there are other advantages.

Mr. Ries:—Then my understanding of the matter is correct in assuming that each engine is only of sufficient power to operate *one* of the two dynamos at a time, and that it is only in case of the disablement of *one* of the engines *and* the *opposite* dynamo that the combination alluded to is supposed to be utilized.

Mr. Arnold:—Suppose the battery should be out of service and one engine should become disabled. You would then be obliged to have the full capacity of both generators to operate the plant. The way these engines are designed and piped, you can double the capacity of either engine, and drive both generators to their full capacity from either engine, while the breakdown is in existence.

Mr. Ries:—Isn't this construction somewhat complicated, as against the use of a separate engine and dynamo held as reserve?

Mr. Arnold:—No, it is not complicated, costs very much less money than a third unit, and gives the same reliability.

Mr. Ries:—Then the magnetic clutch, to which you referred. I suppose that it is intended to disconnect the disabled engine from the main shaft, leaving the internal shaft to drive both dynamos?

Mr. Arnold:—Yes, sir.

Mr. Ries: – And as to the magnetic clutch connecting the intermediate shaft?

Mr. Arnold:—That is a double clutch whereby you can connect either generator to its corresponding engine, or you can also connect the interior shaft to either engine, or connect either generator to the interior shaft. It works both ways. I want to state that I do not wish too much stress to be put upon this magnetic clutch question at this time, because I have not yet developed it to a point where I am able to say what it will or will not do; but I thoroughly believe in such clutches, and am now engaged in developing a formula to build them by. The other mechanical connections are now in operation, and they are successful.

Mr. Ries:—I simply wanted, Mr Chairman, to get a little further explanation as to the reasons for this peculiar construction.

Mr. Arnold:—There are a number of advantages to this thing, which, as the plant increases in size, become apparent. I have not said anything about them in this paper. The paper

was prepared simply to show the general plan of the plant, and this arrangement being somewhat of a hobby of mine. I have mentioned it as little as possible, except enough to give the general idea of its make up.

Mr. Douglass Burnett:—We are bound to admire the care which Mr. Arnold has bestowed upon all the details of this plant, and observing that a great deal of information is available, I desire to draw upon him for a little of it.

First,—as to any trouble he may have experienced with motor load; is not a large proportion of the output between 11 A. M. and 1 P. M. absorbed by motors, and does he find it desirable at that time to run separate engines for the two classes of service—lighting and power? Under those panicky conditions to which he refers, what is the maximum number of amperes which he might be called upon to supply?

The car mile consumption of his elevators has been given on page 273 as $4\frac{1}{4}$ K. W. hours. I very much wish that that number of K. W. hours could be translated into cents; in other words, what does current cost him per K. W. hour?

Finally, as to the load curve: How does this theoretical diagram work out in practice? Does it coincide substantially with one which would be obtained by observation? We presume that it was designed for a winter's day,—that is, maximum condition, and not for the average of an entire year. We trust that Mr. Arnold will enlighten us upon these points.

Mr. Arnold:—The black line represents the total load of the plant on an ordinary day. The dotted line above represents the maximum load of the plant on a dark day. In other words, our Chicago weather is so uncertain that it very often happens that the sun may be shining, and suddenly a cloud will come up and darken the sky over the city, so that all the lights in the building will need to be turned on. It is under those conditions that the upper dotted line was prepared. In that case the plant would be at full load, but the surplus would have to come from the second generator or battery auxiliary. That is another case where the second generator would be run by the opposite engine. The average light day load is shown by the dotted line immediately under the heavy line; the other being the maximum or dark day load. Both engines and generators will handle the maximum day load conveniently. One generator and the battery auxiliary will handle the maximum load without difficulty.

I hope to be able to get the cost for fuel down to a sum not exceeding $1\frac{1}{2}$ cents per K. W. hour. At some stations we are doing it at four-tenths of a cent per hour, where bituminous slack coal is worth ninety cents per ton. I think I will be able to show the Institute, in six months from now, a record of cost of coal per kilowatt hour of one cent from this plant. Under those conditions, four and a quarter cents per kilowatt hour means four and a quarter cents per car mile of travel on the elevators.

Adding to that the cost of labor, I think I will show you a record of running those elevators at a cost not exceeding eleven or twelve cents per car mile, whereas it is now costing eighteen to twenty cents per car mile for hydraulic elevators. In this connection I will state that I have tested a number of electric elevators in Chicago, for one of the large elevator corporations, and I find that the consumption of energy per car mile of travel varies from four to eleven kilowatt hours. I hope to be able to operate these cars at a cost of not over six cents per car mile for fuel alone. This is nothing but what anybody could do who would take these conditions and study them and make the most of everything available.

MR. BURNETT :—We thank Mr. Arnold very much. However, we wish he could state the actual maximum current required to run the elevators in that building. I should also like to ask if he can give us the total kilowatt hours generated during a year.

MR. ARNOLD :—I do not know that there is any hesitancy on my part in giving that information. The current taken by the two machines now in operation,—I mean the starting current, when starting a live load of 3000 pounds, together with the weight of the car,—reaches as high as 600 amperes at 125 volts. That is the service. It should be borne in mind, that the particular elevators there in use consume no current on the down trip; consequently the average consumption of current per car mile is low as compared with some elevators which consume current both going up and coming down, although not so high a starting current. The figures of $4\frac{1}{4}$ kilowatt hours per car mile are correct, because they are taken from a wattmeter which has been running now for four months. Indeed, that is the only figure that I felt absolutely safe in giving regarding the operation of the plant at present.

MR. LIEB :—I am sure that we all appreciate the careful preparation of Mr. Arnold's paper. I can only express the hope that when the plant has been in operation a sufficiently long time, Mr. Arnold may keep his promise and give us in some detail the operating costs. The question of the cost of current production in a plant of this character is an important one, and there is a great lack of reliable information sufficiently detailed to be of use in making comparisons. Usually many of the important items of cost are left out of consideration, and, if Mr. Arnold will permit me, I would suggest that in making up his analysis for future presentation to the INSTITUTE, he might with advantage follow the lines of some of the blanks used for that purpose by the large illuminating companies, which give the items of cost under appropriate heads. I think such a paper would be a valuable contribution, and the discussion would bring out important and interesting data. There are not many plants of the size of the one described by Mr. Arnold, operated as an isolated, plant and which have such a combination of elevator load, lighting load, and motor load. The arrangement of gener-

ating machinery, which he has adopted in his installation, would give valuable data for comparison, and for my part I hope that Mr. Arnold will fulfill his promise, and in due course of time present to the INSTITUTE details of operating cost.

MR. RIES:—It strikes me that the most valuable feature, probably, of Mr. Arnold's proposed installation is the extended use which he makes of secondary batteries in connection with the dynamo. Some years ago, I had occasion to devise a system somewhat analogous to this, for railway work, and from the revived interest which has been manifested of late in the secondary battery, I think it will be but a short time before the battery is very largely used both in stationary service and in railway installation. I notice, on referring to the diagram, Fig. 6, that the battery is very largely drawn upon between 11 and 1 o'clock in the day time, and between 11 o'clock at night and 7 o'clock in the morning. This would indicate from the abruptness of the lines that the battery is switched onto the service mains independently of the dynamos. But I would like to ask Mr. Arnold whether he also uses the battery as a regulator to render the load on the dynamo continuous or practically uniform while the dynamo is running and supplying these various forms of service?

MR. ARNOLD:—I use two regulators, one a hand regulator, and the other an automatic regulator. The hand regulator is adjusted by the engineer at a proper point, so that a certain number of the cells are used in parallel with the elevators; or, rather, so that the total voltage of whatever cells are used in parallel with the elevator is just enough less than the voltage of the dynamos to allow the cells to receive a constant charge, except when the maximum pull comes on the elevators. At all other times the batteries are in parallel and are receiving a charge. Then the regulated cells are charged by means of the generator end of the motor-generator, and the entire series of cells is charged by means of the generator end of the booster running in series with the main dynamo.

MR. RIES:—The batteries are in parallel with the dynamos, and in case of any sudden fluctuation of load, the batteries supply the deficiency?

MR. ARNOLD:—Yes, sir.

[At this point the President resumed the Chair.]

THE PRESIDENT:—Gentlemen, is there any further business to bring before the meeting?

THE SECRETARY:—We have an invitation from Mr. George Hill to visit the plant of the American Book Company on University Place. We also have an invitation from the Crocker-Wheeler Electric Company to visit their works. Both have already been announced.

On motion, the thanks of the INSTITUTE were tendered to the National Electrical Exposition Company for the privilege of occupying their convention hall, and for courtesies extended.

[Adjourned.]

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, September 23, 1896.

The 108th meeting of the Institute was held this date at 12 West 31st Street, and was called to order by President Duncan at 8.30 p. m.

The President:—The Secretary has some announcements to make.

Secretary Pope:—At the meeting of the Executive Committee of the Council, August 5th and September 23d, the following associate members were elected:

Name.	Address.	Endorsed by
Abella, Juan	Director General of Public Lighting, Buenos Aires; residence, 691 Calle Bolivar, Buenos Aires, Argentine Republic.	W. G. Whitmore. J. C. Bennett. M. L. Mora.
Appleyard, Arthur E.	Manager and Engineer, Natick Gas and Electric Co., Natick, Mass.	Geo. W. Blodgett. Robt. B. Taber. I. N. Farnham.
Barry, David	Electrician and Superintendent, Amherst Gas Co., Amherst, Mass.	J. B. Cahoon. C. B. Burleigh. Giles Taintor.
Bell, Oro A.	Electrical Engineer, Western Electric Co., 22 Thames Street, New York; residence, 921 St. Nicholas Avenue, New York.	H. F. Albright. Geo. A. Hamilton. J. J. Carty.
Betts, Hobart D.	Member of Inspection Dept., The Edison Elec. Ill'm'g Co. of N. Y.; residence, Englewood, N. J.	J. W. Lieb, Jr. C. R. Agnew. C. L. Eidlitz.
Biddle, James G.	Mfrs' Agent and Importer Scientific and Electrical Instruments, 944 Drexel Bldg, Philadelphia, Pa.; residence, 264 Rittenhouse St., Germantown, Pa.	E. G. Willyoung. Herbert Lloyd. R. W. Pope.
Bolan, Thomas V.	Supervising Engineer, The Gen'l Electric Co., Schenectady, N. Y.; residence, 869 N. 41st St., Philadelphia, Pa.	H. G. Reist. Ernst Berg. Chas. P. Steinmetz.
Brayshaw, I.	Telegraph Inspector Great Southern Railway, Buenos Aires.	Henry Jackson. W. H. Preece. R. W. Pope.

CARPENTER, CHAS. E.	Vice-President, Carpenter Enamel Rheostat Co.; residence, 36 W. 35th Street, New York.	H. F. Albright. G. A. Hamilton. Ralph W. Pope.
CODY, L. P.	Manager and Engineer, Grand Rapids Electric Co, 9 South Division Street, Grand Rapids, Mich.	Chas R. Cross. Frank B Rae. A. F. Walker.
DARROW, ELEAZAR	Superintendent, Cincinnati Edison Elec. Co.; residence, 220 W. 8th St., Cincinnati, O.	J. A. Cabot. Thos. J. Creaghead. L. G. Lilley.
GRANBERY, JULIAN H.	Draughtsman, with Post & McCord, 289 4th Ave., N. Y.; residence, Closter, N. J.	Clayton W. Pike. A. A. Knudson. R. W. Pope.
GREENLEAF, LEWIS STONE	Electrical Expert, The American Bell Telephone Co., 42 Farnsworth St.; residence, "The Ludlow," Clarendon St., Boston Mass.	Chas. R. Cross. Russell Robb. Theo. Spencer.
HADLEY, FRED'K W.	Electrical Engineer, West End St. R'way Co., Boston; residence, Arlington Heights, Mass.	Chas. R. Cross. Wm. L. Puffer. Chas. F. Scott.
HALL, J. P.	Electrical Contractor, 143 Liberty St., N. Y.; residence, 200 W. 136th St., N. Y.	E. S. Keefer. J. Hatzel. Frank A. Pattison.
HATHAWAY, JOSEPH D., JR.	Assistant in Cable Dep't Western Electric Co., 22 Thames St., N. Y. City.	H. F. Albright. G. A. Hamilton. M. E. Canfield.
HILL, NICHOLAS S., JR.	Engineer of the Electrical Commission of Baltimore, 508 Equitable Building, Baltimore, Md.	Louis Duncan. Joseph Wetzler. Henry Morton.
HUGGINS, N. W.	Salesman, etc., General Electric Co., Seattle, Wash.	S. Z. Mitchell. F. L. Dame. F. B. H. Paine.
KING, VINCENT C., JR.	With V. C. & C. V. King, 517 West St.; residence, 110 East 16th St., New York.	Joseph Broich. Joseph Sachs. R. W. Pope.
LABOUISSE, JOHN PETER	1625 Thalia St., New Orleans, La.	Brown Ayres. P. R. Middlemiss. Chas. K. Huguet.
LORIMER, GEO. WM.	Superintendent of Construction, The Callender Telephone Exchange Co., Bradford, Canada.	Romaine Callender. Joseph Wetzler. Otto A. Moses.
LORIMER, JAMES H.	Superintendent of Erection, The Callender Telephone Exchange Co., Brantford, Canada.	Romaine Callender. Joseph Wetzler. Otto A. Moses.
MACLEOD, GEORGE	Superintendent and Engineer, Kentucky and Indiana Bridge Co., 29th and High Sts., Louisville, Ky; residence, New Albany, Ind.	G. W. Hubley. R. W. Pope. W. D. Weaver.
MAKI, HEIICHIR	Chief Engineer, Kioto Traction Co., 39 Washio St., Kioto, Japan.	Chas. R. Cross. Wm. L. Puffer. Albert Schmid.
MAXWELL, EUGENE	Superintendent, Third Street and Suburban R'way Company, Seattle, Wash.	S. Z. Mitchell. F. L. Dame. F. B. H. Paine.

McKittrick, F. J. A.	Graduate Student Cornell University, 89 Heustis Street, Ithaca, N. Y.	Edw. L. Nichols. Fred'k Bedell. C. P. Matthews.
Morgan, Chas. H.	Student Lehigh University; residence, Maxatawny, Berks Co., Pa..	Alex. Macfarlane. J. H. Klinck. Frank M. Tait.
Nock, Geo. W.	Chief Engineer, in charge of Steam and Electric Plant, Westinghouse Elec. and Mfg. Co., Pittsburg, Pa.	Philip A. Lange. L A. Osborne. Leo A. Phillips.
Parker, Lee Hamilton	Ass't Engineer Railway Dept., General Electric Co., Schenectady, N. Y.	W. B. Potter. John B. Blood. Edw. M. Hewlett.
Pike, Alex. Rea	In charge Testing Dept., Missouri Electric Light and Power Co., 20th and Locust Sts., St. Louis, Mo.	Geo. H. Morse. A. L. Reinmann. Francis Jehl.
Rideout, Alexander C.	Principal of Commercial and Telegraph Dep't Hillsdale College, Hillsdale, Mich.	B. J. Arnold. W. M. Stine. R. W. Pope.
Sampson, F. D.	Manager, Charlotte Electric Light and Power Co., Charlotte, N. C.	Herman S. Hering. A. M. Schoen. C. K. Stearns.
Satherberg, Carl Hugo	Mechanical Engineer and Chief Draughtsman The MidvaleSteel Co., Nicetown, Phila. Pa.; residence, 1752 N. 26th St., Philadelphia, Pa.	Clayton W. Pike. Carl Hering. C. Billberg.
Scott, James B.	Electrical and Mechanical Engineer, 227 East German Street, Baltimore, Md.	H. A. Foster. P. O. Keilholtz. C. G. Young.
Thayer, George Langstaff	Manager, Belle Plaine Electric Light Co., Belle Plaine, Ia.	Ludwig Gutmann B. J. Arnold. Carl K. MacFadden
Van Vleck, John Falconer	Constructing Engineer, The Edison Elec. and Illuminating Co. of New York; residence, Glenridge, N. J.	Edwin J. Houston. A. E. Kennelly. J. W. Lieb, Jr.
Whitaker, S. Edgar	The General Electric Co; residence, 93 High Rock Avenue, Lynn, Mass.	Chas. R. Cross. Wm. L. Puffer. Elmer E. Boyer.

Total 37.

ELECTED SEPTEMBER 23d, 1896.

Name.	Address.	Endorsed by
Brinckerhoff, Henry Morton	Electrical Engineer, Metropolitan West Side Elevated R.R.; 258 Franklin St., Chicago, Ill.	A. V. Abbott. W. M. Stine. B. J. Arnold.
Meadows, Harold Gregory	Associate Engineer (Elec.) with Newcomb Carlton, 109 White Building; residence, 114 West Chippewa St., Buffalo, N. Y.	C. A. Adams, Jr. Edwin H. Hall. C. W. Ricker.

NEWBURY, F. J.	Manager Insulated Wire Dep't John A. Roebling's Sons Co., Trenton, N. J.	M. M. Davis. F. W. Roebling. R. W. Pope.
PINKAS, JULIO,	Director General, State Telegraphs and Telephones, Sucre, Bolivia.	R. W. Pope. Max Osterberg. A. A. Knudson.
RICHARDS, CHAS. W.	Partner, Cumner-Richards Co., 69 Broad St., Boston; residence, Needham, Mass.	A. B. Cumner. F. E. Cabot. G. A. Wardlaw.
WHITE, CHAS. G.	Public Schools Sup't., and Instructor in Physics and Chemistry, Lake Linden, Mich.	R. W. Pope. W. D. Weaver. Wm. J. Hammer.

Total, 6.

At the request of the President, Mr. Charles P. Steinmetz, Vice-President, took the Chair President Duncan then delivered his inaugural address on the "Present Status of the Transmission and Distribution of Electrical Energy," as follows:

Inaugural address of the President at the 108th meeting of the Institute. New York, September 23d, 1896. Vice-President Steinmetz in the Chair.

PRESENT STATUS OF THE TRANSMISSION AND DISTRIBUTION OF ELECTRICAL ENERGY.

BY LOUIS DUNCAN.

The industrial life of mankind is made up of two things. The transformation and distribution of material, and the transformation and distribution of energy. The raw material from mines and forests is changed to finished products and distributed among the people, while energy, obtained from water power, coal or other sources, is changed from the potential energy of the water, or the energy of chemical combination to mechanical power, heat, light, etc. Unless we can transmit this energy economically, we must transform it into the required form at the place where it is to be utilized. At present a large part of our mechanical power is obtained from steam plants situated in the factories themselves, and for heat and light we mainly depend upon stoves and lamps in our houses.

Before the introduction of electrical transmission, it was possible to distribute energy to limited distances by various methods, but no system offered a long distance transmission for all purposes. By means of compressed air or steam pipes the energy of coal has been transmitted to produce mechanical power or for heating, and gas mains have allowed the distribution of gas for lighting or for fuel.

In the case of power obtained from steam plants, the economy incidental to large units and a steady load, has led to the concentration of industries. Where steam is used, the plants are situated where it is most convenient for manufacture. Where water power is employed it is necessary to bring the factories to the location of the power irrespective of other conditions.

By means of dynamo electric machines, the energy obtained from either coal or water power may be transformed into electrical energy; may be distributed and then transformed again into mechanical power, light or heat, or may be used for a number of purposes peculiar to this form of energy alone. The limits to the distance of this distribution are imposed by conditions of economy and safety.

It is my purpose to take up the different methods of transmission and distribution, and to consider the limits that are actually fixed by the present status of electrical development. The question is a commercial one, each problem presenting different conditions which must be considered, but certain general principles govern each case, and our knowledge and experience make it possible to judge the practicability of each particular transmission.

Generating Plants.

At the present time practically all of the electrical energy distributed is generated in plants operated either by steam or water power, and it is important to consider the conditions of maximum economy in large generating plants, as this bears directly on the subject of transmission and distribution.

A large proportion of the electrical plants in this country are steam plants. In the last ten years we have advanced from small stations using high speed dynamos for light and power distribution, to large stations, using, as a rule, low speed direct connected machines. The simple engines that were used some years ago have, in many cases, been changed to compound and even triple expansion engines, and where it is possible condensers have been employed. Some of the latest plants have machinery of the highest possible efficiency, and yet if we consider the price per H. P. of the power generated, we shall find that it is greater than we expect. This is partly due to the fact that for both lighting and power purposes the load on the station, is, as a rule, not uniform, and the apparatus is not working under the best conditions for economy. In this country electrical energy is principally generated for electric lighting, for electric traction, and for supplying stationary motors; the stationary motors, as a rule, being supplied with current from lighting stations. If we take the load diagram of such stations in large towns, we will find that the average output is not greater than 30 to 40 per cent. of the maximum output. We have, therefore, to supply a large amount of

machinery corresponding to the maximum demand on the station, while for distribution a large amount of copper is required that is only being used at its maximum capacity for a comparatively short period of the time. In stations supplying power for traction purposes, we find a variation of load, but the variation is a different kind from that found in a lighting station. In the latter the load varies at different hours in the day, but for any particular instant it is practically constant. In the former the average load for different hours during which the station is operated will be practically constant, but there will be momentary variations depending upon the size of the station and the type of traffic. Taking for instance a 2000 H. P. station in Baltimore, I find that the average load is 48 per cent. of the momentary maximum load. This difference in the kind of variation for the two types of stations, necessitates employment of different apparatus to obtain the maximum economy for each type. For lighting stations triple expansion engines may be used, while for traction work, where the variation in the load is sudden, and may occur after the steam is cut off from the high pressure cylinder, it is not well in general to go beyond compound engines, and there is even a question as to whether simple engines are not more economical when condensing water cannot be obtained. In any case, however, it is of the utmost importance as regards economy of operation that the load should be made as constant as possible.

Two distinct types of distribution are used for incandescent lighting in this country—the single-phase alternating current and the direct current three-wire system. At the present time the former does not permit the supplying of power. As alternating distribution is at high potential it does permit the location of the station where the conditions of maximum economy can be fulfilled. The three-wire incandescent system using low voltages may be used for supplying motors, but the amount of copper necessitated by the low pressure has caused such stations to be located near the centre of distribution irrespective of the best conditions for the economical operation of the plant.

With the alternating system it seems impossible to provide even a moderately steady output, but with the continuous current system the motor load during the day gives an average output greater in proportion to the maximum. Some years ago the question of the relative values of the alternating and direct current systems was discussed, and for a while most of the stations

installed were of the alternating type. At present the tendency seems rather in the direction of continuous current stations, especially in towns where there is a large demand for current within a comparatively small area. There is a great advantage of direct currents in that they allow the employment of storage batteries, which equalizes the load on the station. In almost all of the large lighting plants, both here and abroad, this plan has been adopted to a greater or less extent, and the results have been so favorable that the battery equipments in many of our stations are being increased. The efficiency of batteries in lighting stations is comparatively high, while the depreciation has been greatly reduced, and is not now over five or six per cent. per annum. In most systems, however, the full benefit of the storage batteries is not realized, as the batteries are placed in the station, and while the advantage of an approximately constant load is obtained, yet the further advantage offered in distribution is not secured. I will take this question up later.

In New York, Brooklyn, Boston and Chicago, a large proportion of the direct current lighting stations are situated where it is expensive to handle the coal and ashes, and where the economy, due to condensation, is not obtained. It is also the custom to use several stations instead of a single large station, and this increases the cost of production both in operating expenses and fixed charges. The question arises whether we have reached a point where it will be more economical to consolidate the stations in the best possible location for economical production of energy, and make use of the means of distribution which have been developed in the last few years to increase the radius at which energy can be supplied.

As far as traction stations are concerned, their efficiency and output would be increased by the use of batteries, both because the machinery would be steadily loaded, and because the most efficient type of apparatus could be used as is the case in lighting stations. By the consolidation of railroad properties that has taken place in the last few years, single corporations operate electric lines over extended areas. It is the custom to build a number of stations, each running a certain section of the line, the idea being that the decreased cost of copper and the decreased possibility of a shut-down would more than compensate for the increased cost of operation and fixed charges. It is, again, important to consider the question whether we have not reached the

point where a single station can be built in such a way that there is little or no possibility of any accident causing a suspension of the entire traffic of the system, and where improved methods of distribution will decrease the amount of copper so that it will not exceed that required by the present method of using a number of generating stations.

If storage batteries are used, the two types of variable load belonging to lighting and power stations demand different types of battery. For lighting stations a considerable capacity is required, while the momentary variations of power stations do not require any great capacity, but demand as great a maximum output as battery manufacturers can obtain.

In water power plants the conditions of economy are different. The location of the plant is, of course, definitely fixed, and the advisability of obtaining a uniform load, by means of batteries, depends upon the local conditions. If the water power is limited, and is less than the demand, then it might be well to use batteries in order to increase the amount of salable power. Again, if the development is expensive, it might be cheaper to develop a smaller amount of power, pay for a smaller amount of machinery and increase the output by the addition of batteries. These are questions that can only be decided by a knowledge of the local conditions.

We may conclude that while the practice in large lighting and traction systems is to multiply stations near centres of consumption, yet the economy of a single large station makes it important to consider whether it is not possible to concentrate our power at some point where the expenses will be a minimum, and distribute by some of the methods which have in the last few years proved successful and economical. It is important to make the station load steady, and this may be done for continuous current lighting and traction plants, by means of storage batteries.

Electrical Distribution.

The distribution of electrical energy to consumers as distinguished from its transmission to long distances, has been largely accomplished by the agency of continuous currents, although alternating currents have played an important part in incandescent lighting. As I have stated, a considerable proportion of current for lighting is distributed at constant potential on the three-wire system, or at constant current on arc light circuits, while power

for traction circuits is distributed at approximately constant potential at an average of say 550 volts.

I shall first consider the condition of affairs in a traction system in a large city, where a number of suburban lines are operated. If direct distribution is attempted from a single station, it will be found that when the distance exceeds five or six miles a large amount of copper must be employed to prevent both excessive loss and excessive variation of potential on the lines. On suburban lines it is the latter consideration that usually determines the amount of copper used, and this is especially true on lines where there is a considerable excursion traffic. Even in the city itself, the supplying of sections at distances three or four miles from the station may require so much copper that it would be less expensive to operate separate stations. Several methods other than the direct method may be employed to remedy these difficulties. For outlying lines where the traffic is mainly of the excursion order, being variable both during the day and for different seasons, boosters may be advantageously used. It is perhaps best from reasons of economy to run the boosting dynamos from motors. These dynamos are series-wound and are connected to feeders of such resistance that the fall of potential in the wire for a given current is compensated for by the rise in voltage of the booster. There is a decreased cost of copper incidental to this system, due to the fact that the drop is not limited by considerations of regulation—the voltage at the end of the feeder being constant—while the transmission is at an increased potential. If the average station potential is 600 volts, and it is boosted 300 volts, then the copper for a given loss would be decreased in the ratio of 36 to 81. The booster system has the advantage of the direct system when the cost of the additional apparatus together with the increased loss on the line, capitalized, is less than the increased cost of the copper necessary to produce the same result by the direct system. Whether the balance is in favor of one or the other depends on the distance and the variation of the load, and it is indifferent whether the variation in the latter occurs often or not.

If any transforming device is employed to feed a distant section of the line it must be remembered that the capacity of the device must be great enough to look out for the maximum demand on this section. Suppose now that we wish to feed some suburban line where the load has considerable momentary fluc-

tuations, but where the traffic is moderately constant during the year. In this case the booster could be used with a storage battery at the end of its feeder, the battery supplying the line. The advantages of this combination are greater than with the simple booster, and in many cases they will compensate for the interest and depreciation on the battery and the loss in it. If the arrangement is properly made, the load on the booster and line wire will be practically constant, thus decreasing the capacity of the booster to that required for the average load, while less copper will be required for a given loss. As to the latter point, suppose a given amount of power is to be distributed in 24 hours, say 200 amperes at 600 volts, if the load is uniform, the loss will be proportional to $200^2 \times 24$ hours. If it is all distributed in 12 hours, the loss will be proportional to $400^2 \times 12$ hours, or twice as much. So in the case of the steady load, the same power could be transmitted with the same loss with half the copper. It makes no difference whether the variation extends over 12 hours in 24 or occurs every other minute, the result will be the same. It is apparent then that it is of the utmost importance to keep the line steadily loaded, as well as the station, and this points to the location of the battery near the points of consumption and not in the station. By this system—a booster with storage batteries —it is possible, assuming the same loss, to transmit power to a distance of ten miles with approximately the same amount of copper that would be required for a five-mile transmission on the direct system. It would increase the economical radius of distribution twice, and the area of distribution four times. A single station could economically supply lines within distances up to ten or twelve miles. If it is desired to still further increase the radius of distribution, it is possible to do this by employing some of the alternating current methods that have come into use. I will discuss these methods later, but at this point I may remark that the use of stationary and rotary transformers permits the energy to be transmitted in the form of alternating currents, and to be changed again into continuous currents of any required voltage. These rotary transformers supplied by an alternating current which is transmitted from the station at a high voltage, may be used to feed the line directly, or they may be used to supply storage batteries which are connected to the line. In the latter case we have the advantage of decreased size of apparatus, of steady load on the station, and of a minimum cost of copper on the line;

which system it would be best to employ would depend upon the distances and the character of the line and load.

Of the systems that I have proposed for city and suburban distribution from a single station, three have been successfully employed, namely: the booster system; the booster system with batteries, and rotary transformers operating directly on the line. When we consider the advantages of a single station and a steady load, it seems evident to me, that many of the large traction systems would do well to concentrate their stations into one, and to use the booster system with batteries for their outlying lines, and if necessary use rotary transformers for lines beyond the limit of ordinary suburban work. As to the possibility of the complete shut down of such a station, we have reached such a point in the construction of machinery, both electrical and mechanical, that with a proper reserve, a careful system of duplex steam piping, and with fire-proof construction of the station, such a possibility may be disregarded; while the batteries would look out for any momentary interruption on the feeders.

Continuous Current Low Voltage Distribution.

Some of the most important stations supplying incandescent lamps are operated on the three-wire continuous current system. In the last few years a considerable advance has been made in the sale of power for motors from these stations, and this has increased the revenue and has given better average output. The tendency in this country has been in the direction of using storage batteries in such stations, and abroad practically every continuous current station uses batteries. As in the case of traction systems it has been the custom in large cities to build a number of separate stations instead of building a single plant and distributing from it. The batteries have been placed in the stations themselves, and no attempt has been made to decrease the amount of copper used by employing a number of centres of distribution and giving the main feeders a steady load. The same considerations that apply to stations for traction work will also apply to stations used to supply lights, and the same methods of distribution may be used. It would unquestionably be more economical, in many instances, to use single stations, to transmit power from these stations to centres of distribution, where batteries may be located and to distribute from these centres on a three-wire system. A case in point is the system used at Budapest, where the energy

is distributed from the central station to rotary transformers at sub-stations, these rotary transformers feeding batteries, current being distributed from these batteries on a three-wire system. The reports of the operation of this station show that it is both economical and successful, and it might well be copied by some of the companies in this country. The gross receipts of some of the large illuminating companies bear such a large proportion to the company's stock, that a comparatively small saving in operation would mean a considerable increase in the dividends, and there is no doubt in my mind that by using one power station, with battery sub-stations for distribution, that the operating expenses can be considerably decreased.

Alternating Currents for Lighting.

Alternating currents have been employed for lighting in this country, and they have been especially valuable where a district is to be supplied in which the distances are considerable as compared with a number of customers. It has been almost the universal custom to supply small transformers for each consumer, and while the average size of transformers is greater now than it was a few years ago, yet they are comparatively small. No power has been supplied from such stations, and although alternating arc lamps are used to a limited extent, yet the number is not increasing, and in some cases continuous current arc lamps have been substituted for the alternating. Under these conditions the load on the station is even more variable than in the case of a continuous current supply where motors may be employed, and the constant loss due to the large number of small transformers used, places this system at a disadvantage as compared with the continuous current system. The great advantage it possesses lies in the increased area of distribution rendered possible by the high voltages that are used, together with the possibility of locating the stations where power can be cheaply made. Abroad in the last few years, most of the new stations that have been built use continuous currents, although some years ago the greater proportion of them were alternating current stations. It is also the custom abroad to use sub-stations with large transformers for distribution, thus doing away with a considerable part of the constant loss due to the small transformers used here. It is not possible, at the present time, without greatly complicating the system, to obtain a steady load on the station, and the

only question that arises is the value of sub-stations, and the possibility of using some form of alternating current other than the single-phase.

Methods of Electrical Transmission.

Coming to the question of transmission of electrical energy as distinguished from the supply to customers from distributing centres, there have been great advances made in the last few years, and these mainly through the introduction of multiphase alternating currents. Single-phase alternating currents permit the transmission of power to long distances and its distribution for lighting purposes. It is also possible to supply power from such circuits to large motors working under a steady load. It is not possible, however, to distribute power economically for ordinary uses. As most long distance transmission schemes contemplate the substitution of electric motors for steam engines, and as their success will, in many cases, depend upon the possibility of such substitution, single-phase alternating currents are not at present able to comply with the conditions imposed by the desired service. The introduction of multiphase alternating systems, where two or more alternating currents are employed, the currents differing in phase, has completely changed the situation with respect to long distance transmission. I shall consider briefly the possibilities of such systems, and their value as compared with any direct current system.

Continuous Current Transmission.

The first long distance transmission plant was operated by the continuous current system, and even now plants are being built in which continuous currents of high potential are used to transmit energy to distances up to 15 miles. As compared with transmission by means of alternating currents, we will find that the continuous current system possesses some advantages and some disadvantages. If we consider the relative cost of the copper in the line for a given amount of power transmitted, and for a given maximum potential between the conductors, we will find that the relative amounts for the continuous current and the different alternating current systems, will be as follows:

Continuous Current	100
Single-phase Alternating	200
Two-phase "	200
Three-phase "	150

We see then that the continuous current has a marked advantage over the alternating current systems as far as the cost of copper is concerned. There are, however, certain practical disadvantages belonging to this system. The high voltages necessary for long distance transmission make it impossible to distribute the current at the receiving end without first reducing the voltage. With continuous current this can only be done by employing a rotary commutator of some kind. A plan which has been practically and successfully used has been to run a number of dynamos in series at the generating end of a line, while at the receiving end are a number of motors, also arranged in series, which are used to drive other generators to give the required type of current and the desired voltage. It has not been found possible to make either dynamos or motors of any great output, as there are practical difficulties in running dynamos of high potential where the current taken from them has a considerable value. M. Thury, has installed a number of continuous current transmission plants that have apparently given excellent results. At Biberist, a transmission of 15 miles is employed. At Brescia, 700 H. P. are transmitted over 12 miles at a maximum of 15,000 volts. M. Thury states that generators for 45 amperes can be constructed up to 3000 volts, and he thinks that 4000 could be successfully used. These machines, however, are small when compared with the 5000 H. P. dynamos in use at Niagara, for instance, and where the transmission is a large one the great number of machines necessary would be a serious objection to this type of transmission. It will be seen that the greatest possibility of trouble, in such a transmission, lies at the ends of the line, in the generating and receiving apparatus. It is necessary, no matter what our voltage is, that both the dynamos and motors shall be directly subjected to it, and this with commutated machines will always be a source of danger. If we are to do any considerable amount of lighting from such a station, our energy for this purpose undergoes three transformations before it reaches the lamps, and the efficiency would not be so high as in a corresponding alternating current system. It would hardly be possible to supply motors for ordinary work at the high voltages used for transmission, and the current for them would have to be transformed in the same manner as the current for the lamps. It must be recognized, however, that this system has been successfully used and has given excellent results in a few cases of transmission. Its great advan-

tage lies in the decreased amount of copper as compared with the alternating systems, and in the absence of induction effects, which are a drawback to alternating current transmission.

Transmission by Alternating Currents.

A large proportion of the transmission plants that have been installed in the last few years have been of the alternating current type. These have, as a rule, given satisfactory results, and the installations that are now being erected or planned are almost exclusively on an alternating current basis. The great advantage of this system lies in the fact that it is possible to change the voltage of the current without the use of rotating apparatus, and at once economically and safely. Low voltage dynamos may be used, the voltage may be increased in any desired ratio by stationary transformers, the energy may be transmitted at an increased voltage, and at the receiving end the voltage may again be reduced by transformers. If we compare this method with the continuous current system, we will see that to obtain an alternating current of the required pressure at the receiving end of the line, we would use the same number of transformations required by the continuous current system. We have the great advantage, however, that our changes in voltage have been obtained by the agency of stationary apparatus, which is much cheaper, is more efficient, and is safer than that required in the continuous current system. It is possible to increase the voltage by means of transformers to almost any value with perfect safety, and with an efficiency as high as 98 per cent. or 99 per cent. If then our alternating current, when it has been reduced at the receiving end, is as valuable for distribution as the current obtained by the direct current system, there will be no doubt that alternating transmission has great advantages over continuous currents.

I have spoken of the relative amounts of copper required by the single-phase, two-phase and three-phase alternating currents. I do not think it necessary to explain minutely the difference between these systems, as they are well understood. In a single-phase system a single alternating current is used. In a two-phase system two alternating currents, whose phases differ by 90 degrees, are employed, while in the three-phase system, there are three currents differing in phases by 60 degrees. I shall consider the characteristics of these three systems, as there has been much discussion

especially as to the relative value of the last two of them for transmission work. I shall not discuss the various modifications of the systems, but shall confine myself to general considerations. There is no single-phase motor in successful commercial operation that does not require to be started from rest by some outside means. This prevents a single-phase current from being used at the present time for power distribution; and as, in most transmission, the distribution of power is an important item, single-phase currents are not suitable for this purpose. In a two-phase system the currents are usually carried on separate pairs of wires, while in the three-phase system, three wires are generally used, a common return being unnecessary as the sum of the currents is zero, unless the circuits are unbalanced. In distributing on the three-phase system, a fourth wire can be employed, as it gives an advantage in the amount of copper used.

In all these alternating systems the great difficulty lies in the fact that the inductance of the circuit causes the current to lag behind the electromotive force. This decreases the amount of energy transmitted by a given current at a given voltage; it causes a drop in the voltage of the line, and it increases the armature reaction of the dynamo for a given current. The total inductance of the circuit is made up of the inductance of the transformers, of the dynamos, of the receiving apparatus and of the line. In the case of transmission to very long distances the line inductance is a large proportion of the total, while the inductance of the receiving apparatus depends upon whether lights or motors are to be supplied and upon the construction of the latter. When the different wires of the multiphase system are fed from windings on the same dynamo armature, then the drop in voltage due to any excess of load on one of these circuits cannot be compensated for on the dynamo itself. If the amount of current and the lag of the current is the same for all of the circuits of the system, then it is easy by a compounding winding of the dynamo, or by changing the current in the field winding, if there is no compounding, to keep the voltage constant at either the sending or receiving end. When the load on the different wires of the system is not the same, however, it is, as I have stated, impossible to keep all of the circuits at the proper voltage. Where a two-phase transmission with separate circuits is used, then if the separate circuits are wound on different armatures, each can be regulated to give a constant voltage at the receiving end. This

is the case, for instance, in the large dynamos built by the Westinghouse company for use at the World's Fair in Chicago. The difficulty due to the uneven loading of the circuits is specially marked in the case of the three-phase system, and it is one of the principal objections that have been urged against the employment of this system for distribution. It should be pointed out, too, that it is not enough to balance the quantities of current for the three-branches of the system, but the character of the current must also be considered. A non-inductive load on one wire, with an inductive load of equal value on the others would cause an unbalancing just as if the currents differed in amount. In most of the transmission plants that are being operated and that are proposed, it is required to run both lamps and motors from the same circuits, and while a slight variation of potential on the motors would not cause any particular trouble, yet the successful operation of the lamps requires a practically constant voltage. I think, however, and the same grounds have been taken by others, that in any practical transmission of considerable size, it is possible to so balance the loads that this difficulty will not exist to an extent to cause any serious trouble. When the distributing part of the lines is reached, it is usually the custom when a three phase transmission is used, to employ four instead of three wires. As for line inductance in the two-phase and three-phase systems, there is no question that the latter has an advantage in this respect. By suitable arrangement of circuits the line inductance can be brought to a minimum, and this is of the utmost importance in long distance transmission. I will not take into account the supposed increased efficiency of three-phase motors and dynamos as against two-phase apparatus, as there is a question as to whether a superiority exists, but simply considering the decreased amount of copper required and the decreased inductance of the line, there is no question, in my mind, that for transmission, the three-phase system is superior to the two-phase. It is well known, of course, that the inductance of the circuit can be, in some measure, compensated for by the use of condensers or over-excited synchronous motors. The first of these remedies is, however, a very uncertain quantity commercially, while the second should be used as much as possible, that is, as many synchronous motors should be connected as is practicable. The best remedy, as things stand at present, lies in the careful construction of the line and the apparatus, so that the effects, although they exist, can be reduced to a minimum.

It has been shown by Mr. Scott, and others, that it is possible to transform a two-phase into a three-phase current, to transmit it and to transform it back again to a two-phase current. This will allow us, if we wish, to use two-phase dynamos for generating the current, to transmit with the advantage incidental to the use of three phases, and at our reducing end to use two-phase circuits for transmission. This has some advantages as far as balancing the voltage on the circuits go, and it has been proposed in the case of several plants whose installation is being considered.

Looking broadly at the value of alternating transmission as against continuous current transmission, we have a gain in the simplicity and safety in the transmission, and at the distributing end the use of multiphase currents enables us to supply both lamps and power with an economy and success comparable to that of the continuous current system. If it is necessary to use continuous currents for certain types of distribution at the receiving end, they can be obtained by the use of rotary transformers, by which the alternating current is transformed into a continuous current. These machines have approximately the efficiency of corresponding continuous current dynamos, while the output for a given size is about 50 per cent. greater.

Possible Voltages and Distances of Transmission.

A number of calculations have been made as to the possibility of transmitting electrical energy to very long distances. If the question of cost of transmission alone is considered, then where water powers or culm heaps are within distances of 100 miles of some large centre of consumption, it has been shown that it would be profitable to generate and transmit electrical energy. In these calculations, however, voltages are assumed that have never been employed for commercial plants, and whose availability is problematic, while sufficient stress is not apparently laid on the question of the reliability of the power. If the industries of a large city depended upon a single transmission plant, it is evident that the question of reliability is of paramount importance. Where energy is supplied to manufacturers, to street car systems, and for lighting, a break down that would involve the cutting off of current for a day would mean an enormous pecuniary loss to the community. As the distance of transmission increases, the possibility of accident is increased in greater ratio because we have not only the higher voltages to control but

the length of the line that must be looked out for is also increased. The best guide lies in the practical experience which has been obtained in the present transmission plants and the consideration of the difficulties that have arisen and the remedies that have been employed. I have prepared a partial list of the principal transmission plants that are now in operation.

Name.	Type.	Distance in Miles.	Line Voltage.	Horse Power.	Remarks.
Ouray, Col	Direct,	4	800	1200	Successful, increasing.
Geneva, Switzerland	"	20	6600	400	Successful.
San Francisco, Cal	"	12	8000	1000	Successful, 9 years.
Brescia	"	12	15000	700	
Pomona and San Bernardino	Single phase Alt.	13½ to 28¾	1000	800	Successful, increasing, 4 yrs.
Telluride, Col	"	3	3000	400	To be increased, 3200 H. P.
Bodie, Col	"	12½	3400	160	Successful.
Rome, Italy	"	18	6000	2000	Increasing to 9000 H. P., 3 yrs.
Davos, Switzerland	"	2	3660	600	Successful.
Schongeisung, Germany	"	4½	2600	820	"
Springfield, Mass	2-phase Alt.	6½	3600	820	"
Quebec, Canada	"	8	5000	2130	"
Anderson, S C	"	8	5500	200	"
Fitchburg, Mass	"	2¼	2150	400	"
Winooski, Vt	3-phase,	2½	2500	150	"
Baltic Conn	"	5	2500	700	"
St. Hyacinthe, Canada	"	5	2500	600	" 2 years.
Concord, N. H	"	4	2500	5000	" 2 years.
Fresno, Cal	"	35	11000	1400	" to be increased.
Big Cottonwood to Salt Lake City, Utah	"	14	10000	1400	"
Lowell, Mass	"	6 to 15	5500	480	"
Sacramento-Folsom, Cal.	"	24	10000	4000	1 year.
Redlands, Cal	"	7½	2500	700	3 years, extending lines in other towns.
Lauffen to Frankfort, Ger	"	100	30000	300	(Experimental.)
Lauffen to Heilbronn	"	9	5000	600	Successful.
Oerlikon Works, Zurich, Switzerland	"	15½	13000	450	"
Portland, Oregon	"	12	6000	5000	"
Silverton Mine, Col	"	4	2500	400	" to be increased.

It will be seen that the longest transmission is at Fresno, Cal., the distance being about 35 miles. The highest alternating voltage used is 13,000 volts at Zurich, Switzerland. The highest direct potential is 15,000 volts at Brescia.

All of these plants are working successfully, and this fact will lead to still longer transmission and higher voltages. No limit of either distance or potential has as yet been reached. If we consider the record of the present transmission plants, we can safely say that it would not be going outside of the safe limit of development to transmit at least 50 miles at a potential of 20,000 volts, provided the energy could be delivered at such a price as to be considerably lower than the cost of a corresponding amount of

energy obtained from a steam plant. This, of course, is a matter of local condition entirely, and the commercial value of such a transmission will depend upon local conditions.

Long Distance Transmission for Railroad Work.

The possibility of long distance electric railroad lines is intimately connected with the possibility of long distance transmission of power. We have seen that it is possible to transmit considerable distances from a single station. The current so distributed is not, however, such that it can be applied directly to railroad motors, but it must be transformed at points along the line, the distance apart of these points of distribution depending upon the system that is employed. At present continuous current motors are used, and considerations of safety would lead us to use line potentials not greater than 700 volts. By distributing rotary transformers at distances of five or six miles apart, we would be able to supply motors with current without any great investment in copper. The amount of copper required, could be still further reduced by using rotary transformers with storage batteries thus keeping a constant load on the transmission line. It will be found, however, that on any long distance railroad line, the load on any section of the line is exceedingly variable and the discharge rate of the batteries will have to be very high in order to prevent excessive cost for our reducing stations. It is doubtful whether we have reached a point in battery construction where this system of transmission would be economical. It is certain, however, that when the distances are comparatively short, say within 15 miles, and where the traffic is not evenly distributed, that rotary transformers, with or without batteries, can be economically employed for railroad work.

Conclusions.

My conclusions, subject always to the influence of local conditions, are as follows:

1. In both direct current lighting and traction systems, where the power is generated in or near the area of distribution, it is best to use one station situated at the most economical point for producing power.

2. In the case of the traction systems, when the economical area of direct distribution is passed, boosters should be employed directly or in connection with batteries, to a distance of ten or

twelve miles from a station, and beyond this, rotary transformers whether with or without batteries, should be used.

3. In the case of direct current lighting systems, the energy should be transmitted to storage batteries situated at centres of consumption either directly or by means of a rotary transformer and distributed from them.

4. Where batteries are used, it is best to place them at the end of feeder wires to obtain the advantage of a constant load on the wire.

5. The best system for the long distance transmission of energy, for general purposes, is the three-phase alternating system.

6. Commercial transmissions are in successful operation for distances of 35 miles, and for voltages as high as 15,000 volts.

Experience with these plants shows that the transmission to 50 miles with a pressure of 20,000 volts is practicable, beyond these limits the transmission would be more or less experimental.

Discussion.

Mr. C. P. Steinmetz:—I have been very much interested in the problem of power transmission and distribution, discussed by our President, and while his paper is so complete that I cannot expect to add anything essentially new, still less criticise it, I would like to make a few remarks, especially on the rotary converter system.

In railway circuits, wherever the distance is too great for direct feeding from the continuous current generator, the booster is recommended for lines of moderate length, the rotary converter for greater distances. It is probably not generally appreciated that the rotary converter is not a mere transformer from alternating to continuous current, but shares with the booster the feature—valuable in a railway system—of compounding or even over-compounding automatically, or still more in general to maintain any desired continuous current voltage at its commutator brushes, irrespective of load or alternating current generator voltage, within certain limits. While in any stationary alternating current transformer system at constant generator voltage, the secondary terminal voltage must fall off with increasing load, and an automatic compounding of the transformer is impossible, the rotary converter shares with the synchronous motor the feature that by varying its field excitation the voltage can be controlled by means of a variation of the phase relation between current and E. M. F. Thus at no load, the rotary converter can be under-excited, so that the counter E. M. F. is below the impressed E. M. F., and thus the current lagging greatly, and the

E. M. F. of self-induction of the whole system thrown more or less in opposition to the generator voltage. The potential is thereby reduced to the desired no-load voltage. With increasing load, the excitation of the rotary converter is increased, and at some intermediate load the counter E. M. F. becomes equal to the impressed E. M. F., and the rotary converters act as non-inductive load, giving thus a lesser drop of voltage in spite of the larger current than at no load, while with still higher load the counter E. M. F. of the rotary converter rises beyond the impressed E. M. F., the current is made leading, and the E. M. F. of self-induction thrown more or less in phase with the generator voltage, thus actually raising the voltage, so that in an extreme case the voltage is lowest at generator terminals, and rises, the farther we go from the generator, being highest at the commutator brushes of the converter. By means of a weak shunt field and a very powerful series field, the rotary converter can be made to control the excitation and thus the voltage automatically, and this feature is being made use of to a considerable extent now in power transmissions for railway work.

Such a transmission, interesting by its absolutely automatic control, has been in operation for about two years in Portland, Oregon. The generator at Oregon City feeds directly at about 6,000 volts three-phase into the lines. The distance to Portland is 14 miles. Step-down transformers supply rotary converters of 400 K. W. each, which are greatly over-compounded, while the generators are operated at constant excitation. With an inherent regulation of about 15 per cent. in the generator, a resistance loss of 10 per cent. in lines and transformers, and about 20 per cent. reactance voltage at the secondary terminals, a drop of voltage from no load to full non-inductive load should be expected of about 25 to 30 per cent.; still more at inductive load. Due to the action of the rotary converters, however, the potential at the commutator brushes actually rises from 500 volts at no load to 550 volts at full load, the current being lagging at no load, leading at full load.

This plant is still further interesting by having the rotary converters at Portland operating into the same trolley line, in parallel with turbine-driven direct current generators in Oregon City, 14 miles away.

In low tension lighting circuits, as operated on the Edison three wire system, considerable advantage will be gained by the introduction of the rotary converter. The main disadvantage of the low tension continuous current system is the limited area over which the power can be supplied, due to the comparatively low voltage. This necessitates the location of the station in the centre of distribution, where land, coal and water are more expensive, and requires in cities like New York, Chicago, etc., the operation of a number of stations.

By supplying the generators with collector rings and inter-

linking the stations by high potential lines feeding through step-up and step-down transformers, during the time of light load, and perhaps all through the summer, the smaller stations may be operated as rotary converter sub-stations from the larger stations, and engines and boilers shut down altogether, and started up only during times of heavy load, where all stations may be operated as primary generating stations in the same way as at present.

Going still a step further, all stations may be operated as rotary converter stations by high potential feeders issuing from one alternating central station located outside of the city at some convenient place. The high potential feeders will be supplied with constant alternating voltage, and the control of the whole system effected in the rotary converter sub-stations by lead and lag, eventually, where a very wide range of voltage is required, with the help of stationary induction regulators. Such a system would permit connecting very large motors directly on the alternating lines, and thereby relieve the low tension continuous current system of their reaction.

With the comparison of continuous current and alternating current circuits on the basis of maximum voltage, I can not quite agree. In the disruptive action of high potentials, a certain sluggishness exists, that is, disruption takes place a finite time after the application of the potential. This makes it probable that the disruptive strain of an alternating potential is not proportional to the maximum voltage, but less. On the other hand an alternating electrostatic field may cause deterioration of the insulating material by mechanical vibration, by heat due to electrostatic hysteresis or similar phenomena not met with in continuous potentials. Continuous potentials, however, offer a formidable source of danger in their electrolytic action, which does not exist in alternating circuits. In the absence of comparative tests, it is therefore not safe to compare continuous and alternating circuits on the basis of the voltage, and it is quite possible that according to the circumstances, sometimes the alternating and sometimes the continuous electrostatic strain may be more severe.

It is gratifying to see that the voltage used in transmission lines has been constantly increased. While a few years ago 3,000 volts were hardly considered commercially safe, now 11,000 and 12,000 volts are used extensively, and 15,000 to 20,000 volts discussed. The danger limit is reached in the high potential lines; not in the step-up and step-down transformers. Transformers can be built and operated safely at voltages far beyond anything ever thought of for power transmission. Only a few months ago I was able to reach by stationary transformers a potential of 160,000 volts effective, or nearly a quarter of of a million volts maximum,—by the way, probably the highest alternating voltage ever experimented upon by man, if we leave out electrostatic

charges and oscillatory discharges as limited power phenomena, while in my case I had practically unlimited power,—a 100 K.W. motor—behind the 160,000 volts. In line insulation considerable progress has been made, and insulators can now be secured which will not be pierced below 50,000 or 60,000 volts effective alternating potential in dry weather. When damp, in fog or rain, a considerably lower voltage will leak or creep over the insulator surface and thus short-circuit the line, and this brings us to the real limitation of transmission voltage which exists at present: the climate. In a perfectly dry climate I should not hesitate to consider 20,000 or even 30,000 volts quite safe, while in a very damp and foggy climate, in rain and sleet, half this voltage may be decidedly unsafe.

MR. TOWNSEND WOLCOTT:—Some little time ago, I think about three years ago, an eminent English engineer referred to the continuous current as a nearly obsolete system, especially so far as distant transmission is concerned, and some of my acquaintances here thought that I was very much behind the times because I could not see it quite in the same light. My position was, without placing any limits on the future possibilities of the alternating current, at that time the continuous current was certainly very far from being obsolete. It is interesting to note this evening that the highest voltage which is mentioned as being in successful commercial operation at the present day is continuous current.

MR. RICHARD LAMB:—Mr. President, I have enjoyed listening to your admirable address, and I would like to make a few suggestions in reference to a detail of long distance transmission. It seems, in the consideration that I have had of the matter, that the main trouble has been commercial—the question of cost of the copper. The alternating current has, by your paper, been advocated for long distance transmission, and it has one feature which I believe will in the near future, cause it to be used almost exclusively for long distance transmission, and that is the skin effect. In Prof. Silvanus Thompson's text-book he states that in a thousand cycles, only one-seventh of the diameter is pierced, and that the greater the number of alternations, the less the proportion of the diameter that is pierced. It follows, that with sufficiently high frequency, after giving due consideration to the amperage to be transmitted, a large, comparatively thin copper tube will make the best conductor.

Now, one of the long distance transmission difficulties, is that of heat in the cable. If we have our large diameter tube, we have a greater area subject to the atmosphere to cool. In some of the experiments that have been recently made in Europe, I understand that simple tubes have been used to very great advantage in distributing alternating currents. They have been comparatively large tubes so far as the diameter is concerned.

I have been told by one of the largest manufacturers in the

country, that he can make his tubes in sections of 100 feet and that they will be sufficiently strong, supported at 100 feet distances, not to be blown down if properly clamped. They will stand considerable strain. These tubes would be made by Mannesmann's process.

European tests have shown that Prof. Silvanus Thompson's experiments were correct, and that with an experimental tube line, using a small alternating motor, they got satisfactory and encouraging results by comparison with a plain copper wire. I believe that in the feeder system, for long distance work on the Erie Canal, copper tubes will be used. A test will be made looking to that object.

I would like very much to hear any comments that the President may care to make upon that subject.

MR. ELIAS E. RIES:—Mr. President, I have listened with a good deal of interest to the valuable paper presented to the meeting this evening, and particularly so because I, myself, have given the subject a great deal of attention. About eight or ten years ago I commenced some experiments with alternating currents with a view to their application to railway work, as it was perfectly clear to me even at that time that if electric railways were to be extended over long distances, if electricity was ever to take the place of steam on trunk line roads, or even if it were to be extended into the suburbs of cities for the operation of ordinary street railways over more than the average municipal distances, that higher transmitting pressures must be used than the direct-current 500-volt systems were employing, and I gave the subject, as I have stated, considerable attention, and subsequently took out what I believe to be the first patents that were ever granted on the application of alternating currents of various types to railway work. At that time and for a long period afterward, the trend of popular feeling on the subject of alternating currents for railway or any other kind of power transmission was rather backward, and I am very glad indeed to hear from the evidence presented in the address to which we have just listened, that during the past few years the opinion held by prominent electricians on that subject has changed. Now, there were several methods referred to in this paper whereby the line losses to which existing electric railways are subject could be minimized and by means of which various types of transmission could be employed, and it was said among other things, that a combination of the direct and alternating current systems could be employed, the alternating being used for transmission, and by means of rotary converters transformed into a direct current at lower potential; also that the three-phase and two-phase alternating current systems had certain fields before them for long-distance work, with or without the use of a secondary battery for maintaining the load factor constant, despite the heavy fluctuations that exist in railway work. The employment of a secondary bat-

tery for this purpose, when properly applied, offers an excellent method for the maintenance of the greatly-to-be-desired uniform load, and, in connection with direct-current motors, affords probably the best means for bringing about a maximum degree of economy in copper cost for a given system of distribution, as well as the maximum efficiency in the operation of the generating plant. It is, however, not necessary that the battery be located on the line in order to accomplish this result, since the desired end can in many cases be effected to better advantage, in my opinion, by carrying the battery upon the cars. This latter plan has certain additional advantages to commend it which I will not now take the time to discuss. As to the relative merits of the single and multiphase systems, however, my own opinion is that, owing to the great simplicity and flexibility of the single-phase alternating current, it is more than likely that that will be the ultimate system to be employed in future for electric power transmission on railways. Considerable progress has been made in recent years in the direction of operating single-phase alternating current motors and making them self-starting. I have done some work in that direction myself and I do not think I am saying too much when I predict that within a comparatively short time we will have at our disposal motors of that type which will be capable of successful railway work, being self-starting and furnishing a reasonably satisfactory torque, even at slow speeds which will compare favorably with the work performed by direct-current railway motors to-day. If we are ever to operate long-distance electric railways economically, we must run up the transmitting potential as high as the state of the art in insulating our currents will permit, and then transform them down to a safe working potential at sub-stations or transforming vaults along the line of way, at which ordinary alternating current converters are placed, the lower pressure secondary terminals of which are in connection with the railway conductors. For the time being it is possible and very probable that a great deal of work will be done by converting the alternating transmission current into a lower tension direct current through rotary transformers, as has been said, and that is not, in my opinion, an altogether objectionable system. It is better than being compelled to waste a great deal of copper such as we are in the habit of doing to-day, and it possesses the somewhat questionable advantage of enabling us to use the direct-current railway motors to which we are at present accustomed. But the tendency of modern times is towards simplicity. The question of transmission of electric power for railway work will be no exception, and there can be no doubt that in the present case all the advantages are on the side of a purely alternating current system throughout.

I do not think it is necessary for me to add anything further on the subject this evening, as it has been very ably and fully discussed in its various bearings. But it is a matter that interests

the INSTITUTE and the public in general very largely and I trust that other members will in the near future bring up the matter and throw such additional light upon it as their experience may permit them to do.

MR. NELSON W. PERRY:—The question of distribution of energy in municipalities, that is short distribution, as distinguished from transmission of energy over long distances presents quite a different problem. The two cases are entirely different and distinct. For instance, we find that in long distance transmission of energy, the transmission of that energy in the potential form is usually the cheaper way. I think, Mr. President, you said in one of your former papers that the transmission of coal was effected to-day at about half a cent per ton mile. Now we find in our large cities that while we have got our coal to the depot or to the water front at a cost of say half a cent per ton mile, we are paying fifty cents perhaps per ton mile to get it from the water front to our central station. It costs fifty-seven cents a ton to deliver coal on the sidewalk outside the Duane Street station of the Edison Electric Illuminating Company of New York and to carry away the ashes, and that is a distance of perhaps a mile and a half, not more than two miles, from the water front. Now in respect to distribution in a large city, it is necessary with our low potential system of course to locate our central station in the centre of the district to be supplied where real estate is high. That means that our fixed charges are large, and one of the largest charges in the cost of our energy is the fixed charges. It becomes necessary then that we should reduce those fixed charges to the lowest possible amount. With our present methods I believe it is fair to assume that the boiler-room occupies about the same space as the dynamo and engine room do; that is they are about half and half. The gas engine is a recourse which I have advocated. It would do away with the boiler-room which takes fifty per cent. of the floor space. It would require a little more attendance perhaps than the steam engine, but the stand-by losses would be reduced, and it would permit the location of our plants on property which was cheap and where fuel could be had with the minimum of handling. I recently had occasion to make a few calculations as to the cost of distribution of energy in the potential form of gas. Assuming a gas of .55 specific gravity and with a calorific power such that 25 cubic feet consumed in a gas engine per hour would give a horse power hour under fair conditions, I got some results which astonished me, and which will doubtless astonish others who have not looked into the matter. Assuming a transmission to a distance of 5,000 yards, gas of a specific gravity of .55, and a pipe 12 inches in diameter, and to provide for bends, that there was a 90 degree bend every 200 yards, I have figured out the percentage of power consumed in the transmission under various degrees of water pressure. Under one inch of water pressure we would transmit 500 horse power of

such a gas, the percentage of the power transmitted consumed in transmission would be .007 of 1 per cent. Then going to 10 inches of pressure we would deliver 1600 horse power with a loss in energy of transmission of .07 of one per cent. of the power delivered. The percentage loss in transmission with gas is very nearly directly proportional to the pressure, whereas you know that with electricity it is the other way, and the two curves would cross each other somewhere—just where, would depend upon the conditions. While the gas transmission for municipal distribution would seem to be by far the more economical method, it would not apply at all to the longer transmissions because of the direction of these two curves which would be straight lines approaching each other at a somewhat obtuse angle. The plan which I had in mind was the location of a large plant for commercial purposes on land which was cheap, on the water front or on a railroad where the coal could be handled at a minimum of expense, and the ashes be gotten rid of with very little trouble, and the transmission of that fuel gas to centres of distribution where gas engines would be employed to drive dynamos. Those centres would preferably be centres of smaller radii than usually supplied by central stations, or if for any reason it seemed desirable to increase the radius of these districts, and even with the shorter ones, it might be advisable as President Duncan has said, to use the storage battery in order to use the machinery, and plant, and labor that we did employ to the best advantage by operating them continuously at their best output. But it seemed to me that these figures of the cost of transmission, the drop in transmission by gas, is something that is astonishing. I had no idea that I would arrive at any such figures as those when I started to make these calculations. In electrical transmission when the distance is not very long we allow 10 per cent. Five per cent. would not be economical in many cases, and here we go down, in one case, to the seven thousandth part of one per cent.

Mr. J. G. White:—I presume that most of the people present are electrical engineers rather than gas engineers, and are consequently interested in seeing the electrical side of this question fairly upheld. I would like to ask Mr. Perry one question, and that is, what allowance he has made for leakage in his gas transmission, and what allowance it would probably be necessary to make for leakage, assuming that the energy was electrically transmitted. In the gas transmission, so far as I know the subject, the leakage is by large odds the most important item, and the loss in pressure or loss of energy due to transmission is practically negligible, whereas directly the reverse is true with the electrical transmission. I know of a gas plant which is now in operation and distributing its produce through some three miles of pipe, the most distant point being within two miles of the holder, in which the average loss due to leakage is about 43 per cent. Suppose we add to that the .007 of one per cent. for

loss in transmission; then we get a very respectable total. So large an allowance would not be necessary with a well constructed line—one that was kept in good repair; but it would even then be a very considerable item.

Mr. Perry:—I would say that I had not allowed for 40 per cent. leakage in this estimate of mine. A friend suggests that the leakage referred to may have been in a municipal plant.

The President:—The transmission was certainly very economical; but everyone who has seen an Italian pushing a lot of dynamite in a push-cart must have observed that there was a great deal of energy transmitted with very little expenditure of power. We would hardly, however, consider a push-cart a particularly efficient means of power transmission.

Mr. Steinmetz:—On single phase alternating current motors, inventors have been working since the early days of alternating current distribution, that is for about ten years, and on an average about three or four times every year we have been told by some inventor that the single phase motor is practically perfected, and that we should wait a few months only to see a perfect motor brought on the market,—and we are waiting still.

In the meantime our theoretical and practical knowledge of alternating current phenomena has been vastly increased, so that we can fairly well judge now on the prospects of self-starting single phase motors.

The self-starting single phase motor of the induction type is not a mere fantasy any more, but such motors are listed as standard apparatus by manufacturers, and are in commercial operation. At present, for instance, I am building a 100 H. P. single phase induction motor to go on a 60 cycle lighting circuit, and to start with considerable torque.

It must be understood, however, that the single phase induction motor compared with the polyphase motor must necessarily be less efficient, have a lower power factor, and a larger exciting current, and require a larger current to start with the same torque. That is, if in a polyphase induction motor the efficiency averages from 75 to 94 per cent., according to the size, the power factor from 85 to 90 per cent., the apparent efficiency from 60 to 80 per cent., the exciting current from 20 to 35 per cent., and the motor will start with full load torque, while consuming not more than full load current, the single phase induction motor will give only from 65 to 85 per cent. efficiency, from 70 to 85 per cent. power factor, from 50 to 70 per cent. apparent efficiency; it will have an exciting current from 35 to 60 per cent., and require from two to three times full load current to start with full load torque, or develop from one-third to one-half full load torque with full load current in starting. Indeed a very satisfactory and efficient single phase motor can be produced, but the same motor operated as a polyphase motor would be still better.

The single phase synchronous motors are not self-starting, but

have to be started by some means, either as polyphase motors by phase splitting devices, or by means of commutators, in very small motors, or by a small induction motor or direct current motor. When running at speed, however, they are practically as good and efficient as polyphase synchronous motors, and produce no lagging currents, but when properly excited give 100 per cent. power factor, and can even be made to compensate for lagging currents, by over-excitation.

Such motors have been used for power transmission, and the oldest power transmission by alternating currents in this country, the Walla Walla plant,—still in operation, I believe,—consists of a pair of 60 K. W. high-frequency alternators, one as generator and the other as motor. The method of starting the plant is quite interesting. The generator has a 500-volt exciter, the motor a 110-volt exciter. The line consists of two wires in parallel. In starting, the 110-volt exciter of the motor is started as continuous current motor over one of the lines by the 500-volt exciter of the generator, and starts the synchronous motor. When up to speed, the motor is synchronized with the generator over the second line, then the continuous current starting line is disconnected and thrown in parallel with the other line.

Other alternating single phase motors, as series and shunt motors or commutator motors in general, need hardly be discussed, since they have proved a dismal failure even in such small sizes as fan motors. As a starting device, the commutator is used on synchronous motors to a limited extent.

Mr. Ries:—I wish to state that I succeeded some time ago in constructing a synchronous motor that was self-starting. However, I have not arrived at that point where I would recommend that motor for use as a practical railroad motor. It was rather of smaller size and for that reason I did not state that a successful alternating current motor, single phase, self-starting, was now immediately available, but that I hoped and thought that it would be very shortly. I am glad to note, however, that Mr. Steinmetz is willing to admit, as we most of us know, that there are in use successful single-phase alternating current motors, even though their efficiency as compared with the two-phase motor or with a direct-current motor may not be very large. However, the point must not be lost sight of in discussing the transmission of power for railroad work, that in using a two-phase or three-phase motor it renders necessary the use of two or more overhead trolley wires or other line conductors instead of a single one, still using the ground, of course, as the return. That necessity involves objections of so serious a nature that we can well afford to sacrifice something in the efficiency of the motor—a single-phase motor—if by that means we are put in a position to operate our lines by means of high-tension transmission and low-tension working currents as I originally proposed. Another very great point in favor of the single-phase alternating current system that must

not be overlooked is the exceeding simplicity, mechanically and electrically, of the alternating current motor, and its ease of control and freedom from faults and serious breakdowns under excessive loads as compared with the railway motors now in general use. These advantages are sufficient, in my opinion, to more than counterbalance the losses due to their, at present, slightly lower electrical efficiency. It was in that view of the situation that I say that in order to have the greatest simplicity in the system, and simplicity is required if it is ever to be a permanent success, the single-phase alternating current system is the one that will in all probability be adopted. Of course, as in every other direction of development and progress, we must make use of those things we have, before flying to others that we know not of, or at least, have not in immediate hand. The next year or two will probably see a great many long-distance electric railways installed in which rotary transformers are employed. There is no objection whatever to rotary transformers except that they involve the use of moving mechanism, and still retain that undesirable and apparently indispensable commutator; and if you place rotary converters at various sub-stations along the line, one or two attendants at each station are required to look after them and keep them in order. Whereas if you put down a static converter with no moving parts whatever, it can run itself without any attention; you can bury it out of sight in a hermetically tight box or vault or any other suitable receptacle and it requires no attention whatever, and everyone must concede that such a system as that, is by far the most desirable one, and, as I have already said, we can easily afford to sacrifice a little in the efficiency of our motor if by that means we can operate a system possessing so many points of superiority, and so many vastly greater advantages than the direct-current system that we now have, or than the two-phase or three-phase system, with its multiplicity of wires, that has been spoken of.

MR. STEINMETZ:—While I do not share the opinion that single phase alternating current motors will find an application in the near future for railway work, polyphase alternating railway systems will probably be installed to a certain extent soon. They have, indeed, the disadvantage of requiring two trolleys. However, as you all know, one of the largest street railway systems in this country is operated on the double trolley system, and operated successfully, and the objections to the double trolley are very much less on long distance high speed roads, and on suburban and interurban lines, where switches and crossings are few, and such roads would constitute the proper field of application for three phase railways.

Regarding self-starting single phase synchronous motors, no such motor has ever been built. Of course, a single phase synchronous motor can be supplied with a commutator to start as series motor, and will then start, provided that the load is light enough, and the

motor is small enough to come up to speed before the commutator has burned up.

Mr. Wolcott:—I would like to ask for information if there are any polyphase railroads in operation now, except the one at Lugano, Switzerland.

Mr. Steinmetz:—There is no three-phase railway in operation in this country, but in Europe I have heard that one small road is being operated. I have been doing a considerable amount of experimenting during the last two years on three-phase railway motors, with entire success, and in all probability some roads will soon be equipped with such motors.

[Adjourned.]

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, October 21st, 1896.

The 109th meeting of the Institute was held this date, at 12 West 31st Street, and was called to order by President Duncan at 8.10 P. M.

The Secretary announced the election of the following associate members by the Executive Committee at the meeting in the afternoon.

Name.	Address.	Endorsed by,
Benoliel, Sol D.	1327 Broadway, residence, 120 W. 35th Street, New York City.	Geo. F. Sever. F. B. Crocker. W. H. Freedman.
Fish, Milton L.	Ass't Manager, Pasadena Electric Light and Power Co., Pasadena, Cala.	Chas. R. Cross. Wm. L. Puffer. H. V. Hayes.
Rice, Arthur L.	Professor of Steam and Electrical Engineering, Pratt Institute, Brooklyn, N. Y.	Harold B. Smith. A. S. Kimball. H. J. Ryan.

Total 3.

The President:—This evening we have a topical discussion on "Electric Traction under Steam Railway Conditions," to be opened by Dr. Charles E. Emery.

A Topical Discussion at the 109th Meeting of the American Institute of Electrical Engineers, New York, October 21st, President Duncan in the Chair and Chicago, October 28th, 1896, Mr. F. E. Drake in the Chair.

ELECTRIC TRACTION UNDER STEAM RAILWAY CONDITIONS.

(A Topical Discussion.)

Opening Remarks by Dr. Chas. E. Emery.

In accepting an invitation to open this discussion upon general lines, it appeared necessary to make an investigation to ascertain in a general way the outcome of an attempt to introduce electric traction on our trunk line railways, in view of the rapid improvements that have been made in electrical apparatus, and the reduction of cost that has followed the greater demand.

In the light of recent achievements, it can be assumed at the outset that electric traction under steam railway conditions is feasible. The only question is whether it will pay. The present applications only prove the former proposition, but do not touch the latter.

Electric traction on street railways was commercially successful because it gave increased power by comparatively simple means, and the actual cost of operation was less than formerly with horses, so that the saving in this respect, and the increased travel due to better facilities, warranted the enormous development which has taken place in this direction. The application of electric locomotives for suburban traffic underground, as on the City and South London Railway and on a branch surface road to Nantasket Beach, near Boston, with other somewhat similar applications, show the practicability, desirability and financial success of electric traction for such service. The Baltimore tunnel application shows that electric power may be transmitted and utilized in very large units successfully, and the freedom from escaping gases warrants the application at that particular location without further investigation. In suburban traffic a certain flexibility is necessary by which large crowds may be taken when desired; and if the comfort of the transit can be so increased as to make frequent trips desirable to travelers, the business will be built up and the heavy expenditures necessary to change to electricity be compensated for in this way, even on

the present steam operated roads for considerable distances from large business centres. It does not seem probable, however, under present conditions, that any probable increase of travel will warrant the enormous outlay necessary to secure electric traction throughout the whole length of our trunk railroads.

The greatest practical efficiency of an electric system of the kind proposed, between the engines at the central station and the rails would probably be 60 per cent. This, on account of a second transmission to sub-stations and the necessity of using rheostatic regulation to some extent, would probably be reduced to 50 per cent., so that twice as many horse power would need to be generated at the central station as at the track with the present steam locomotives. Each horse-power in the central station will be developed for two pounds of cheap coal, or four pounds per net H.P. delivered, whereas the steam passenger locomotive will on the average require six pounds, based on net tractive force and allowing for the various wastes. The saving in coal due to electric passenger traction will therefore be one-third. Coal is procured cheaply by the railroads, but probably an inferior quality costing 50 cents per ton, or say 25 per cent. less than that used on locomotives, could be employed in the electric stations, so that, for trains of like weight, the saving in *cost* of coal for passenger service would be $\frac{3}{4} \times \frac{2}{3} = \frac{1}{2}$. For freight engines we calculate the saving in cost for fuel will be about 55 per cent., and for switching engines about $66\frac{2}{3}$ per cent. On railroads running through the coal regions the total cost of coal is about 9 per cent. of the total operating expenses. A saving of one-half in the cost of the coal then corresponds to a saving of 5 per cent. of operating expenses. For reasons that will be stated later, it is believed that, for general railroad work, independent electric locomotives will be required, and that these will necessarily be as heavy as present steam locomotives, on which basis the only saving in weight will be that of the tender, which we have assumed as 10 per cent. of the total weight of the train for passenger engines, 3.3 per cent. for freight engines, and 5 per cent. for switching engines. The total load to be hauled will therefore be decreased by these several percentages, and the cost of coal reduced thereby to 45 per cent. of that required by the present locomotives for passenger trains; $43\frac{1}{2}$ per cent. for freight trains, and 32 per cent. for switching trains. Applying these percentages for the relative amounts of coal used for these different purposes on a prominent railroad, the average saving in fuel becomes 58.9 per cent. or 5.89 per cent. of operating expenses. The cost of water is taken at $\frac{2}{3}$ of 1 per cent. on the same basis, which increases the saving to 6.56 per cent.

Considerable savings have been claimed on other grounds. First, in relative repairs of electric motors compared with locomotives. Repairs will be less on the motors of course, but are not inconsiderable, and when we consider that the transmission

line, trolley line and trolleys must be kept in repair as well as the motors, it cannot be far in error to assume that the question of repairs will be about balanced, independent of central station apparatus otherwise provided for.

It has been claimed that labor will be saved because the second man on an electric locomotive is required simply to provide for sickness or accident of the driver, and can well be the baggage-man of a passenger train or the conductor of a freight train. Something of this kind may be worked out on an unimportant country road, but it would be impracticable on a large business scale. If a trunk line were to be changed to electricity to-morrow the best policy would be to keep the old engineers to operate the locomotives and learn of electricity as fast as possible, and to put the young men from the training schools of the large companies as the second men to watch electrical details and overcome unforeseen contingencies, the firemen taking their places as they learn how. It is also claimed that there will be a saving in the weight and cost of electric locomotives, particularly when applied under a car, but this system is of limited application. Heavy locomotives on trunk lines must have the same weight for electric as for steam traction, independent of the tender, as explained hereafter. Large savings in repairs to tracks and bridges are also claimed on account of the smoothness with which electric locomotives having no reciprocating parts would operate. The same weights must be run over the same rails at the same speed, and at least a great part of the wear and tear would be due to inequalities of the track surface which would influence both systems alike. The present locomotives are so designed that, while the wheel loads on the drivers are heavy, the masses which strike blows are comparatively light; that is, they have simple wheels and axles, spring-connected to the frames. This is a very difficult thing to accomplish with an electric locomotive without using exactly the same construction. On a comparatively smooth track, remarkable steadiness of movement may be obtained, if the armatures of the motors are secured to the axles of the driving wheel and the fields supported concentric therewith, but such an arrangement is entirely unfitted for a railroad track in average condition. Springs only relieve the frame. Each pair of wheels and attached motor acts as an enormous trip-hammer on the rails and roadbed the moment the former gets the least out of alignment, by defect in original construction, by settling after rains or from other causes. A connection to driving axles through gearing reduces the blows very materially, but introduces at high speed another difficulty. The vertical movements of the main axle would necessarily cause a change in the angular velocity of the armature during the time of such movement, which, for jars occurring in the fraction of a second, would bring a heavy strain on the gearing and vary the speed of the armature momentarily.

The variations in current due to such variation may also give a longitudinal pulsation to the train. The matter is not helped at all by mounting the motors on hollow axles concentric with the driving axles so that the motors may be spring supported from the axles, for in such case the connection of the motor to the axles must be through links the equivalent of a universal joint, which produce variations in velocity, when the axles are out of centre with the armature, similar to those where gearing is employed. In fact, these very devices are used to obtain variation in angular velocity in many machines. Either gearing or universal joints would have back lash when current was shut off. The suggestion recently occurred to the speaker, and he has not had time to calculate the effect of these considerations, but they must be serious. A train at 60 miles an hour moves about 90 feet a second, sufficient to cover a number of yielding rail joints in that time. When the New York Central Railroad started its fast trains, the engineer found it necessary to go over the alignment with instruments; as dips and vertical curves, which would not be noticed by the trackmen, gave very unpleasant motions to the cars at high speed.

For reasons above given, it will be necessary to mount electric motors for fast locomotives away from the centres of the axles and connect through side rods, as in the present locomotives. The only other method would be to use chains, which are mechanically impracticable. There is another important reason for this. In order to obtain speed the motors must be wound for it so that the counter-electromotive-force will be produced by velocity rather than the number of turns, and, in starting, the motors are necessarily connected in series so as to reduce the starting current. For motors adapted to very high speeds, it will be necessary to put pairs in series even to surmount heavy grades, to act as pushers in case of accident, or in removing snow, etc. The counter-electromotive-force is therefore divided between several motors, but not necessarily in equal degree. If, with motors connected to separate axles, one for any reason slips its wheels, it will monopolize the larger portion of the electromotive force and cut down the current on all the motors in series so that full power cannot be obtained. Working the motors in parallel with an enormous rheostat would be wasteful and, in some cases, impracticable. It is, therefore, important for two reasons to connect driving wheels operated by separate motors by means of side rods.

Again, it is essential that, on the road, speed be regulated by better means than a rheostat. A series motor varies its speed with the load and so cannot run without rheostatic regulation at a given speed if for any reason the number of cars in the train or the track resistance varies. It will, therefore, be necessary to return to commuted series field coils or equivalents, and it will be perhaps desirable to have a shunt winding which can be used

to give still closer regulation. In fact, shunt motors are being introduced abroad for traction purposes. The use of these devices will increase the weight of the motors compared with simple series wound motors, for the simple reason that with the field reduced to the utmost it must still be sufficient to prevent sparking, and there must be sufficient iron employed to make the stronger field efficient when it is desirable to run slower. These considerations make more room desirable for the motors than can be provided in the trucks of an ordinary car.

For these several reasons it is predicted that the high speed electric locomotive of the future will, like the steam locomotive, be a structure independent of the train, that the motors will be hung on the frame independent of the driving wheels, and the same as well as the driving wheels connected by side rods. To obtain proper room under such conditions larger driving wheels will be employed than the wheels of an ordinary car. This will so extend the wheel base that it will not be safe to run at high speeds without the leading truck the same as on an ordinary locomotive, and in fact the electric locomotive will in all its general features be a steam locomotive without the boiler, with motors substituted for the steam cylinders. In this way and probably in no other can the flexibility of the present steam locomotive be obtained. Probably there will be greater difference of opinion on this subject than on any other. The use of motors in the trucks of baggage cars is so fascinating that strong efforts will be and should be made to retain such a system, though the reasons stated are believed to be sufficient to prevent its general adoption. Again, it is desirable that the whole locomotive be a unit, on a strong frame calculated to resist the shocks due to collisions and accidents, and it is doubtful if locomotive drivers will be found who will be willing to risk their lives on any other kind of a structure.

To realize the flexibility of the ordinary locomotive, one has but to go on the line of one of the roads which has not yet adopted the heaviest type of rail, but yet runs express trains at 45 miles per hour, grasp a convenient post close to the line and a little around a curve, if possible, where one can take the wind of the train, and watch its approach. In many cases the locomotive sways nearly a foot from the perpendicular, first one way and then the other. At a bad joint the plunge is so rapid that the effect can be described as terrific, as one cannot but think of the consequences if such a mass should leave the rails. The locomotive, however, follows the inequalities as readily as would a farm wagon. Electrical engineers may insist upon a more rigid and better track, but this will require additional expense and will not entirely overcome the difficulty. The electric locomotive must be constructed so that it will do the work of the present locomotive in the same way, and the feature of flexibility cannot be sacrificed.

A modern heavy locomotive costs about $10,000, which is at the rate of $10 to $12 per H.P., on about the same basis as electric motors would be rated. It needs little argument to show that an electric locomotive to take its place under conditions stated would cost fully as much. Moreover, if like care is to be taken of electric locomotives as of those for steam, the same number must be employed of like capacity for like work.

An approximate calculation of the cost of electrically equipping and operating a trunk line has been based on information in regard to the operating expenses of different railroads, given in the series of articles by Mr. Baxter, in the *Electrical Engineer* early this year. We have the highest respect for the industry and ingenuity of Mr. Baxter, as he was one of those who rendered valuable assistance to the speaker in carrying out a large enterprise a number of years ago, and we have adopted his facts, and for convenience have used some of his methods, but have been obliged to entirely disagree with his conclusions. For instance, we calculate that the cost of the steam and electric generating plants will be about three times as much as he states, the transmission plant and sub-stations about twice as much, and the operating expenses about 5½ times as much as he provides for. Necessarily the conclusions are diametrically opposite.

The calculations are based on the operating expenses of a railway system, comprising nearly 2,700 miles of road and employing 1,800 locomotives. By calculations based on the train miles, checked by the reported coal burned and the probable number of engines in use, Mr. Baxter ingeniously determines an average of 140,000 horse power, considered as continuously operated, which, in our calculations, we estimate will require to replace it 280,000 H.P. in the stations, on the basis of 50 per cent. efficiency from stationary engine pistons to track, and if 60 per cent. can be obtained by commuted fields, or other means herein discussed, the difference simply provides for an expected increase of travel. From the probable average power and the actual reported operating expenses corresponding thereto we proceed as follows: For facility of calculation and to obtain an underestimate, rather than one too large, the power required for the switching engines is distributed among the regular trains on the road. It is also assumed that the average number of trains is continued the entire length of the road instead of using a much greater number than the average for suburban travel. These methods cause an underestimate of the cost of the electric transmission, but enable the cost of operation to be accurately worked up from the averages. The latter is the more important point, as the other merely involves comparatively small questions of difference in the amount of interest. By this generalization the trains will be assumed as separated about 7½ miles, independent of direction, over the whole length of the road for every hour in the year. On this basis there will be required on the average, 106 station

horse-power per mile of road, independent of direction of trains, but to provide for concentrations which will inevitably occur, the generating plants and transmission lines have been worked out on the basis of 150 H.P. per mile. The assumed number of trains will require on the average about 400 H.P. each at track, or 800 at station, which is high for an average, as the power of the switching engines is, as explained, distributed along the whole line for convenience of calculation on the basis of averages. To obtain the economy due to fairly large stations they are assumed to be separated 45 miles from each other, and at two intermediate points transformers and rotary converters (transformers) located, by which means the feeders are supplied every 15 miles. On the above basis it is assumed that 6,750 H.P. is installed at each steam station, and 2,250 H.P. at each sub-station. To avoid overestimates the cost per H.P. of steam and electric plant in main stations has been assumed as only $80 per H.P. with $20,000 for buildings, and for the whole apparatus in the sub-stations there has been allowed only $10 per horse-power and $10,200 for buildings and the copper in the high tension lines. The low tension copper has been worked out on the basis that half way between the main and sub-station two trains may meet, each requiring 1,000 H.P., and that a uniform section of copper sufficient to carry 7½ miles, the current required for half of this power, at an original tension of 700 volts and a drop of 20 per cent., would be ample for the whole length of the low tension lines. On this basis the cost for copper at 13 cents per pound for the outgoing low tension conductors will be $12,386 per mile. It is assumed that provision for supporting the outgoing conductors and the bonds in main track for return current will cost $5,000 per mile. On the basis of these prices, without considering incidentals, the total cost of the electrical generating and transmission plant foots up $31,057 per mile, the annual interest on which at 5 per cent. is $1,553 per mile. If the services of the 1,800 steam locomotives can be furnished by 1,500 new electric locomotives at $10,000 each, the same will cost $5,556 per mile, requiring $278 annual interest per mile, making the total interest on steam and electrical plants, including locomotives, $1,831 per mile. The operating expenses of the station considered as a steam station only, from Emery's tables reduced to 24 hours and 365 days, modifying cost of plant and eliminating coal and interest, is found to be $25.84 per average H.P. per year. The time of 12 extra men for care of electric apparatus in the main and two sub-stations amounts to $2.75 per H.P. per year, which makes the total operating expenses of the generating, transmission and locomotive plants, exclusive of coal and interest, $28.59 per average main station H.P. per year, or $3,031 per mile, or with interest added, viz.: $1,831 as above, a total of $4,862 per mile. The operating expenses of the station thus calculated include an allowance for repairs, insurance, taxes and renewals. It

should be recollected that the cost of coal has been already provided for in the percentage of saving first developed, and that the train expenses are assumed to be the same as for steam locomotives. The operating expenses of the road using steam, amounted to $15,187 per mile. Of this, as previously stated, 6.56 per cent. will be saved by the use of electricity, corresponding to $996 per mile. This subtracted from $4,862 per mile (given as the cost of operating expenses of the generating stations, etc., with interest added), leaves $3,866 per mile per year as the additional expense which will be entailed by the application of electricity as a substitute for steam; so, on the basis that the operating expenses are 50 per cent. of the gross receipts, such gross receipts must be increased 12¾ per cent. by the introduction of electricity over the whole length of the line, in order to enable the road to pay the same dividends as before.

It may be considered that the results will be changed materially by the use of high-tension transmission throughout. If tri-phase currents at a tension of 10,000 volts were received by each electrical locomotive, the tension reduced by transformers carried by the locomotive, and current employed to operate induction motors directly, or to operate direct current motors through rotary converters also carried by the locomotives, the saving independent of extra transformers and converters would amount to $9,714 per mile, corresponding to $486 interest per mile, and reduce the total increased operating expenses to $3,380 per mile, which would require that the gross receipts be increased 10.2 per cent. in order to pay the same dividend as before, instead of 12¾ per cent., for combined high and low tension transmission. The saving in dollars is quite large, but the total costs are so enormous that the saving makes but a small difference in percentage. It will similarly be seen that differences in kind of apparatus employed will have very little difference on the general results, though the savings are important in themselves.

It must be recollected that these results are based on providing electric traction for the whole length of a trunk line. It can hardly be expected that the gross receipts for the whole line will be increased, say, one-eighth by such an application. If, however, the application be made within the radius of suburban traffic such an increase is not only probable, but it may be expected that the cost of operation per passenger mile will be reduced in greater proportion than stated, so that the application of electric traction will pay from the outset. These considerations will apply to longer distances on railroads like the New York, New Haven and Hartford, where the passenger business furnishes the larger proportion of the income.

Again, it is possible to accomplish with the electric locomotive, results that are impossible with the steam locomotive. The power for the former being generated originally in stationary

boilers, or in some localities derived from waterfalls, is not limited, and the power of the motor can be increased indefinitely, so that in particular locations a demand either for greater power to obtain more speed, or a greater or more continued tractive force than is now possible with a steam locomotive, can be met by electricity without difficulty.

On the whole, therefore, although the application to the whole length of long trunk lines does not seem practicable under present conditions, there is no doubt but that the industry will grow in the future as certainly as in the past.

MR. CHARLES K. STEARNS (*communicated*):—The information obtained from the operation of the Nantasket electrical line, during the summer of 1895, was more or less of a general nature, as the chief object in view from the standpoint of the railroad officials was to demonstrate that an electrically equipped road could be operated as satisfactorily in regard to the facility of handling large numbers of passengers on time, as a steam road.

This point was proven beyond doubt, as the railroad officials have expressed themselves as satisfied, after observing the ease with which the trains carried the large number of people the boat line brought to the trains at Pemberton, the extreme end of the line.

The cost of operating this line during its first season was roughly obtained, but the experimental nature of the car equipments was such as to render any figures as to the cost per train mile anything but accurate.

The equipment in the station is no doubt well known; suffice it to say, that there were installed two 500 K.W. direct connected generators wound for 600 volts potential; two compound condensing Green engines of about 1,000 H.P. capacity each, and eight return flue boilers, 18′ x 72″ diameter.

The train schedule of July, 1895, called for 150 trains per week day, or an average of 148.1 trains per day, including Sundays, and in 1896, 66 trains per day, or, including Sundays, an average of 68 trains per day. In 1895 these trains consisted of a sufficient number of cars to accommodate the people, but in 1896 the trains were limited to 2 cars, a motor car and trailer. To accommodate the passenger traffic, extra trains were made up and run between the regular scheduled trains. A fair average on Sundays would be about 150 trains, and week days 75 trains per day. This partially accounts for the difference in the coal consumption in 1895 and 1896, although in 1896 the engines were run condensing, while in 1895, non-condensing.

The line operated in 1895 was 6.86 miles of double track, equipped with special trolley wire, and in 1896 the same length of trolley line, with the addition of 3.64 miles of double track equipped with the third rail. The actual rail laid is about 3 miles double track, allowing for omissions of the rail at the

crossings and stations. The distance from the power station to the end of the third rail is 4.75 miles, and to the end of the trolley line 5.75 miles.

The following table, although incomplete, gives some idea of the operation of the power station during July, 1895 and 1896:

	July, 1895.	July, 1896.
Hours run	$605\frac{1}{3}$	$546\frac{1}{4}$
[1] Ave. Elec. H.P. per hour	245	$349\frac{1}{10}$
Lbs coal burned	629,575	571,100
Coal per E.H.P. hour	4.24	2.99
Ave. trains per day	148.1	68
Ave. cars per train	2.1	2
Maximum cars per train	7	2
Train miles	32,803	44,173
Passengers	267,143	
Tons passengers	18,700	
Tons, dead load	162,089	
Tons total load	180,789	
Per cent. paying load to dead load	10.2	

In regard to the third rail equipment: It must be remembered that this was in the nature of an experiment, and several defects have been noted, which no doubt will be corrected in the future.

The rail itself is about 100 lbs. section, rolled in the form of an angle of 110 degrees, with a standard rail top or surface for the shoe. This rail, as originally installed, was bonded with cast copper bonds, and further supported by insulating blocks placed $7\frac{1}{2}'$ from each end. This was found insufficient, and fish plates were put in with a flexible copper bond and the rail supported by insulating blocks placed as near the ends as possible, with an additional block in the centre to prevent vibration. These blocks are about 6″ high, and fit the angle of the rail. This rail is not continuous, the grade crossings, of which there are several, being omitted, and the train running by momentum over them. This was necessary, as the width of some of these crossings is considerably greater than the length of the motor car. At the stations, also, the third rail is omitted and the overhead wire used. The installation consists, therefore, of a combination of third rail and overhead wire, the train starting out of the stations by the overhead wire and trolley, and running between stations by the third rail and shoe.

I cannot say that this plan is very satisfactory, particularly at night, when the different crossings are emphasized by darkness in the cars. The third rail, as now laid, is alive the entire length.

It seems to me a question as to the advisability of operating a line with a 500-volt difference of potential in a position where there is liability of careless persons coming in contact with it. One accident has occurred and, although I understand legally,

1. During July, 1895, power was furnished by the station to operate the Hull Street Railway, the average of which was 30 H.P. by separate wattmeter. Consequently, the average power for the railroad was 215 H.P. 349 H.P. in 1896 includes about 40 H.P. furnished the Braintree & Weymouth Street Railway, so that the power for the railroad amounts to 309 H.P.

the public are not allowed on the right-of-way, the liability occurs of careless parties or workmen coming in contact with the two rails.

The regular steam rail used is 6″ high. The third rail is 7″ from the tie. This allows a clearance of $2\frac{1}{2}''$ from the pilot of a regular locomotive, provided this pilot is up to standard. There have been several cases where a locomotive pilot, chain or brake-rod on a steam train running over the third rail section, has come in contact with the third rail and caused a short-circuit of such length that it was found impossible to keep the current on this section of the electric branch. This section, I will state, is connected with the power station switchboard by a separate feeder and circuit breaker. In these cases the electric trains have come to a stand-still until the steam train was off the section.

From these considerations it seems advisable to adopt a system where the conductor, if of the third rail type, is alive only at points where it is actually used, or at least divide the line into blocks, so that one train will be in a block at a time.

The insulation resistance of the pole line in 1895 was 140 ohms, or an average leakage of 4.7 amperes. This is on a line consisting of 577 Georgia pine poles, the trolley wire being fastened to angle iron cross-arms, which are bolted to the poles by two $\frac{3}{4}''$ through bolts. No insulators are used. On the third rail there are about 3,500 points of support. I am sorry to say that I have been unable to obtain information as to the leakage on the third rail under varying weather conditions.

A serious objection to the present installation of the third rail is the connection by lead covered cables between the ends of the rails at crossings and stations. The surface leakage from the rail to the lead covering must be considerable, although I am not aware as to whether it has been measured or not. The outside covering of the cables has been found to be very sensibly alive.

Aside from the use of the third rail or trolley wire I believe that more uniformity of load in the station should be obtained by the use of storage batteries either on the cars or in the station. The extreme demands for power on the line, when 75 trains were in service, and these trains heavy, were very noticeable compared with the service of 150 trains per day in July, 1895.

In conclusion, it seems very desirable to put in a third rail in preference to the overhead trolley wire in such a manner as to avoid any liability of injury from accidental contact with the rail. In other words, put in the rail in blocks and then only such part as is in actual use to be active. A method of this kind would reduce the leakage to a minimum and place the electric road on an equal footing with the present block system of steam railroading.

Mr. Charles H. Davis (*communicated*):—The subject of "Electric Traction Under Steam Railway Conditions" can be divided into a consideration of:—

1. *New Roads* to be built.

2. *Old Steam Roads* to be partially or entirely changed over. The relative merits of the use of "Electric Traction" or "Steam Locomotive Traction" in each of the above cases may be considered under the heads:—

First Cost.

Total Expenses { 1. Fixed Charges.
2. Maintenance.
3. Operating Expenses.

Gross Receipts.

1. New Roads to be Built.

First Cost.—The cost of right-of-way, stations, terminals, fencing, grading, ballasting and track, will be approximately the same in either system. Overhead line work, or third rail and feeder system, including track, ground and feeder returns, are not required for steam traction and their cost is an additional charge against electric traction. The cost of equipment, excluding locomotive, is in favor of steam traction, as roughly it may be considered that the motors and controlling devices, or the electric locomotive are the items making up the increase in first cost of electric traction. In the first cost of power plants we can assume buildings to be nearly the same - several round houses as against fewer power houses. The steam and electrical plants, including foundations and stack, will not only exceed the cost of locomotives and tenders per horse-power, but in the use of central electric stations a greater amount of horse-power must be installed. This is especially so when lines are very long, making it necessary to duplicate the central station, or use sub-stations, or both, before it becomes necessary to duplicate locomotives by changing them. Should the headway of trains and their weight and capacity become similar to street railway practice in large cities, then, and only then, would electric power be less in first cost than steam locomotives; if the total power required were large, this might even result in lower first cost of the entire system.

No attempt has been made to give figures, as each individual case must be studied and the difference in first cost determined; but it will in general result in favor of locomotive traction with the exception noted.

Total Expenses.—1. Fixed Charges:—These will be greater for electric traction owing to greater first cost, except in the one case mentioned above.

2. Maintenance:—This subject is too wide to be fully discussed here; but it can be assumed that permanent way, track, car bodies, trucks and other items, the same in each case, would be maintained at equal cost. It is probable that the cost of maintaining the power plant, motors, line, etc., of electric traction would exceed the cost of maintaining locomotives, tenders, etc., and that the saving due to less wear and tear on track and roll-

ing stock in electric traction would not make up for the difference. The liability to break-downs in electrical apparatus is greater than in steam mechanisms, thus tending to increase cost of electric traction, not only on the line, but in the power house. Should the headway be decreased, as already suggested, the comparative cost of maintenance of electric traction would decrease, resulting in favor of such traction, although it may be hard to determine the line at which the saving over locomotive traction will take place.

3. Operating Expenses:—Administrative, legal and extraordinary expenses can be considered the same; train attendance the same, except in the case of close headway and the use of motors under each car, in which case electric traction shows a decided saving. Terminal, station, signal and telegraph expenses can be considered alike in each case. The cost of coal would depend largely upon the amount of power necessary to operate a given road: for the larger the power and more frequent the trains the greater the saving by the use of electric traction. Many central stations assumed to produce a horse-power at lower cost than by smaller units really do not, mainly due to the fact that so large a part of such a station is idle many hours of the day, and, although at their maximum point of economy, they are far ahead of small units, the question at issue is not "the cost of a horse-power at the station," but "the cost of a horse-power at the rim of the driving wheels." The cost of this last mentioned horse-power for electric traction is not only made up of station expenses, but also line expenses and electrical equipment expenses, when compared to the cost of the same horse-power produced by a steam locomotive. When these facts are taken into consideration, it is invariably found that the controlling factor is the headway of trains; the less the headway the more surely will it pay to use electric traction, and vice versa so far as the cost of a horse-power at the rim of the driving wheel is concerned.

A general conclusion would be that the question of total expenses depends so largely upon each individual case that only a study of it will enable one to arrive at a reliable result; but that with light weights, small capacity and frequent service, electric traction can be operated more cheaply than locomotive traction.

Gross Receipts.—The question whether the gross receipts of a given road will be affected by the use of one or the other power under discussion is a most interesting one. Experience shows that where an electric road has paralleled a steam road it has taken most of the latter's business at first, but less as time went on; and that it created a demand for intercommunication which had never existed before—the bulk of the passenger travel coming from this cause. This is interestingly shown in the arguments of Judge Hall (Vice-President N. Y., N. H. & H. R. R.) and Judge Gager, of Connecticut, before the Legislature of the State at its last session. This, of course, refers to passenger re-

ceipts only. Freight receipts would not be affected by the use of one power or the other, they increasing only as the country grows and rates fall, together with better facilities. Receipts from express and mails might be materially increased by the use of electric traction when giving more frequent service. It appears that the close headway and "leave at your door" service of electric roads are the main reasons for their induced travel. The question of how much more the travel would be increased by the use of electric traction and frequent service is problematical, for the "leave at your door" service is wanting in steam railways as they are, but why not change them? If this could be done, past experience and data would give a good basis from which to estimate future results.

The conclusions one arrives at is that for long lines, infrequent service, where freight is a large proportion of the business, and where centres of population are far apart, the steam locomotive is the only paying method of to-day, as the first cost will be less, as well as total expenses. The writer has had several opportunities of determining these facts. What development may bring to electric traction in the far future cannot be foretold.

2. Old Steam Roads to be Partially or Entirely Changed Over.

First Cost.—What has been said under "new roads" will apply, with the additional disadvantage in the use of electric traction due to the increased first cost arising from the necessity of throwing away old steam equipment, either in part or whole. In some cases this would be unnecessary, as with large systems, where existing equipment could be used on that part where electricity did not replace steam locomotives.

Total Expenses.—Remarks under "new roads" will apply; and it would, therefore, appear that unless steam railways can change the character of their service to more nearly conform to street railway practice, they will be in many cases unable to adopt electric traction in place of steam locomotives to their own profit.

Gross Receipts.—What has been said under "new roads" again applies; therefore existing steam roads, to increase their gross receipts, must give quicker and more frequent service, and must give as near as possible the "leave at your door" service.

Conclusions.—The writer believes that electric traction will be profitable to steam railway systems when some or all of the following conditions are fulfilled, depending upon the special problem to be solved:—

1. Steam railway managers must avoid making the mistake which took place in the change from horse traction to electric traction, namely, of trying to reduce the first cost of changing by the use of old methods, material and equipment, which, although entirely suited to the old system, proved most unsuitable under

the new conditions. The tendency is to repeat this mistake, and too much stress cannot be laid upon avoiding it. The old equipment partly made over, the old method of operating, etc., will not bring success in the use of electric traction; and if followed from necessity, would indicate the strongest argument against the change.

2. Long distance, heavy trains and infrequent service, if a necessity, will prevent electric traction being profitable. Therefore, where gross receipts can be increased by light trains and frequent service, thus decreasing expenses as compared to steam locomotives, electric traction will prove profitable. One of the best examples of how this could be applied is found in the suburban service of the Pennsylvania Railroad out of Philadelphia. It is, of course understood, that electric cars can be operated over the same tracks as trains drawn by steam locomotives. A change of system requiring more frequent service for success might necessitate one or more additional tracks, which, in some cases, would delay the time when a change would be advisable.

3. Steam railways, where the second condition is fulfilled, can better the results where they operate part of the system on the "leave at your door" plan. This suggestion may seem to some a radical departure, but I commend it to the careful thought of those interested.

MR. H. WARD LEONARD:—Mr. President and gentlemen, I have made no preparation, and hence have no remarks to make which are very well defined. There is one point that has occurred to me in connection with the remarks of Dr. Emery that I think worthy of comment, although they embraced so many figures it is impossible to speak conclusively off hand. I noticed that in the cost of conductors for the low tension portion of the system, if I understood him correctly, he allowed $12,000 per mile for the copper, and that the trains were located at about 7½ miles apart, and roughly about 500 horse-power to a train. This, it strikes me, is an overcharge, as regards investment—about $85,000 for copper for a train of 500 horse-power—and naturally will make the cost of investment very large against electric railways. One point that I have always believed in, and that it seems to me is likely to be more and more pronounced in the future, is that there must be a conversion of energy before we can operate the electric locomotive at all, and the question arises as to where such conversion should take place. Of course, if we convert our energy and store it, by the use of storage batteries in the station or along the line or on the locomotive, we are, in one way or another, converting the energy that we are using in the locomotive. If we substitute instead of storage batteries in one place or the other, rotary transformers along the line, we are also converting. In the latter instance we are adding very greatly to the cost of the equipment by the fact that each one of these converters along the line must have capacity not merely for one

train, but for the maximum that may get in that section at one time. What I wish to emphasize as my belief in the matter is that the conversion must take place, for the greatest advantage, upon the locomotive itself; that, if storage batteries are to be used they should be placed upon the locomotive. If rotary transformers are to be used they should be placed upon the locomotive. The transmission of power over considerable distances should be done by the alternating system, to secure the best results as regards economy and flexibility, and especially in the case of long distance work, as regards the convenience of securing a large number of feeding points and thereby reducing the cost of any low tension conductors to a very much lower figure than that given by Dr. Emery.

Now the question arises as to what would be the best method of operating the motors, and I think there can be little question to-day that a continuous current for the motors is preferable to the alternating. If it be true that the alternating should be used for the transmission, and that the continuous current should be used for the propelling motors, and that rotary transformers be upon the locomotive, the cost of conductors then, as regards distribution, is limited only by considerations such as would pertain to any long distance transmission—the possible voltage along the line. The conversion could be as cheaply accomplished on the locomotive as alongside of the road. Each transformer, in a case where the transformer is located on the locomotive, would have to take care only of that particular locomotive and train that it was intended for, and no margin need be provided to take care of additional transformation as would be the case in transformers which were outside of the locomotive itself.

As regards the question of possible methods of gearing, I may say that there is one electric locomotive which has run some 2,000 miles covering distances of five and six hundred miles straightaway, which did not meet with any difficulty of the character described by Dr. Emery as to the methods of gearing and connecting between the locomotive and the axle. This is the Heilmann locomotive, which had, in the locomotive which was tested about two years or more ago, a single reduction between the motor and the driving axle. There were no coupling rods—side rods—on this locomotive. It ran at upwards of sixty miles an hour and no difficulty of the nature spoken of was experienced. But on the contrary the motion of the locomotive was conspicuous by its smoothness and entire freedom from all jar. The Heilmann locomotives which have been under construction since the original tests, and which will be in practical service very shortly, have the axle passing through the centre of the motor; that is, there is no gearing, but the driving is accomplished by the armature being connected to the shaft through a kind of spider—something after the nature of the method of driving which we are familiar with by its use in the Short gearless motor

a few years ago. This method of driving will be soon tested and we shall then be able to learn whether such difficulties as have been pointed out will or will not arise. A thing which I do not think enough stress has been laid on in the comparison, is the fact that we have reached what appears to be practically a limit in the size and horse power of steam locomotives; whereas there is practically no limit, as compared to the present limit, in the horse power possible for electric locomotives, and since the thing which is of the greatest importance to-day in railways is to secure, first, higher speeds, and second, greater hauling power, it seems to me that the electric locomotive has a very marked advantage in those lines. Take, for example, the steam locomotive which hauls the New York Central "limited" and which has been pretty carefully tested between here and Albany. The maximum horse power was in the neighborhood of a thousand. The horse power that was developed while accelerating the locomotive to its full speed, and while the speed was at the rate of about thirty odd miles an hour was about 600 horse power. The steam pressure fell very rapidly as the speed increased, that is to say, the mean effective pressure in the cylinder; and this is a trouble which is met with in steam locomotives which will be absent in the electric locomotive. There is a very marked falling off in drawbar pull due to the inability of maintaining a high mean effective pressure in the cylinder of the steam locomotive which would require considerable modification from present types before it can be avoided, it being due to the limitation of rapidity of admission of the steam and one or two other considerations. The maximum weight on drivers that steam locomotives have, is of course the limit to their hauling power, and that very rarely exceeds about 170,000 pounds, and with the electric locomotive there would be no difficulty in making that weight almost anything desired. In the Heilmann locomotives that are now under construction that weight will be 230,000, and that 230,000 pounds will be effective for a perfectly smooth pull. Tests that were made upon the Heilmann locomotive in comparison with the steam locomotive by means of a dynamometer car showed a very marked difference in the smoothness of the drawbar pull. The curve of the electric locomotive as might be expected, was quite smooth, while that of the steam locomotive was very jagged. All this of course is very influential with a very large load behind, in tending to skid the wheels. One good feature of the Heilmann locomotive which ought to commend it, if it has no others as some people seem to think, is that it enables one to study with great accuracy the electric traction problem. The question of speeds and horse power, drawbar pull and all such points as that, can be very accurately studied by means of it. The cost of a locomotive of that type is necessarily going to be considerably higher than that of the ordinary steam locomotive. But the hauling capacity is going to be so greatly increased, and also its possible speed, that it may have a field which would not

pertain to it if it had no advantages in speed and hauling power. The Heilmann locomotive is now being finished and will be tested so soon, that a great many of these points will soon be a matter of fact, and we can secure more reliable data of that nature than we have at present. One point which I think is rarely appreciated is this: That tests upon the steam locomotive show that the efficiency between the indicated horse-power in the cylinder and the horse-power at the drawbar, which, of course, is the efficiency of the machine—the ratio between the horse-power at the drawbar and that in the cylinder, for the steam locomotive operating at about sixty miles an hour, is between 42 and 43 per cent., which, I think, will impress you as quite low. The expectation in the case of the Heilmann locomotives, based upon well known features in the Willans engine and in generators and motors, assuming 90 per cent. for the engine and 95 per cent. for the generators, 98 per cent. for the conductors and 90 per cent. for the motors, will give an efficiency at 60 miles per hour of some 47 per cent. as the ratio between drawbar pull and indicated horse-power in the cylinder. I think we must all admit that the question of attempting to replace the steam locomotive of to-day is an extremely difficult task for the electrical engineer. But it does seem to me that we should draw out as many points as we can, favorable to electric locomotives, and it does not seem to me that that has been done in the remarks of Dr. Emery.

Mr. Geo. S. Strong:—Not being a member of the society, I was invited here to-night by the secretary to discuss this matter from the standpoint of a locomotive man. In doing so, I do not want to be understood as being partial to the locomotive. In fact, I am on both sides, as I am interested in the building of power plants for electricity as well as the building of locomotives. I have studied this subject from the beginning of the electrical operation, and as regards the Heilmann locomotive, I might say that I was the first one to build a motor car on the principle of the Heilmann locomotive, and I have had about $20,000 worth of experience in that line. And as regards the efficiency, I had an electrician who promised me 95 per cent. of the power of the engine which was put on the car. We had a gas engine which indicated and gave a brake horse-power of forty horse-power. The generator was directly on the shaft of the gas engine. The motors were directly under the car with the wire connections very short—not over ten feet of wire in it. I think two mules could have pulled that car faster than that forty horse-power engine could drive it. Now that is my experience with that kind of a motor. I do not say that that cannot be beaten by a properly constructed generator and a properly constructed electric motor under the car, but it led me to believe that some other method of transmission of power would have to be originated before the gas motor would ever become an efficient motor for driving street cars.

Now, as regards the question of the amount of coal necessary to drive a locomotive, Dr. Emery gives six pounds as the average. Mr. Westinghouse has given it as high as eight pounds. I have tested a number of locomotives that ran as low as three pounds. I have a locomotive to-day built, ready to run, on which I will guarantee to give a horse-power for two pounds of coal. I am ready to undertake to build to-day one locomotive or a hundred locomotives and guarantee a horse-power for two pounds of coal, on the axle or on the drawbar behind the tender.

Now when you come to talk about ordinary locomotive practice and what is possible in locomotive practice, and what has been demonstrated as thoroughly practical in locomotive practice, they are entirely two different things. Only two weeks ago at a meeting of the Railroad Club in this room, this very subject was discussed. A paper by Professor Goss was read on the "Results of High Rates of Combustion in Locomotive Boilers," in which he demonstrated that fully 33 per cent. of the coal in an ordinary locomotive goes out through the stack unconsumed, that is, where the grate area is about 21 feet and where the rate of combustion is rushed up to 225 pounds, as I know on some roads it is the regular practice to-day to burn 225 pounds per square foot of grate area. That is entirely unnecessary, and boilers are running to-day in this country which are giving ten pounds evaporation from a temperature of 212, on a locomotive, and there is no trouble in getting a temperature of 212 on a locomotive. There are abundant ways of using exhaust steam to give it, and it has been demonstrated thoroughly and practically that the water can be heated to that temperature on the engine. It has been demonstrated thoroughly and practically, and locomotives to-day are running in regular service that are giving a horse-power on 20 pounds of water—regular work, every day in the week right along. Twenty pounds of water on compound locomotives is not out of the way, and it can be reduced down to 17 and 18 pounds. Now our friend here gives us for this branch of the consolidated road, 2.95 as regular consumption for electrical horse-power in the station. Now, that is with one of the best engines—you know the Green engine. That is one of the best engines there is in the market to-day. You cannot beat it very much when it comes to giving the greatest efficiency for a given steam pressure. Now we all know Dr. Emery states in his paper that we cannot expect more than 50 per cent. of that on the axle from the motor after he had gone through all this immense expenditure to get it there, you cannot get more than 50 per cent. on the axle; so that that engine must necessarily have used in the neighborhood of six pounds of coal per horse power developed on the axle on the car. Now to give you an idea of what you have to contend with—I am not saying this to discourage you at all—in fact I have been through the mill myself; I know how it is to make inventions and how difficult it is

to introduce an invention after you have demonstrated that it is all right. If I were to go to the New York Central with a bond of a million dollars and offered to save one-half of their coal and give them their locomotives if they would give me one-half the saving for five years, they would not listen to me; they would not listen to anybody else. It would not make a bit of difference. They would say: "If you can give us something cheap, that won't cost anything more than we have got to pay, we will try it; but we won't spend any more money. This coal bill is a thing that is going all the time and might be distributed in dividends, but we won't spend any more money." That is the cry. You cannot go and sell anything that costs any more money than they are paying. They want a cheap thing, and when you come down to getting efficiency, that is something that they don't think very much about. Now to give you an idea of what you have got to contend with in the question of handling trains: One estimate here to-night is 500 horse-power as the average for a train. Now as much as ten years ago I took a locomotive that I built for the Lehigh Valley road out over the western roads. On the Northern Pacific we took a twelve car train from a dead stop 10.8 miles in eleven minutes. I indicated the engine myself. She gave a mean effective pressure in the cylinder of 70 pounds in making 326 revolutions a minute, and indicated 1810 horse-power on 1,848 square feet of heating surface and 60 square feet of grate area, 30 horse-power per square foot of grate area and nearly a horse-power for every square foot of heating surface in the boiler. That engine, on the Fort Wayne road, took a 10 car train from Fort Wayne to Chicago, making 23 stops and five slow-downs. The running time for the whole distance was one mile per minute. She ran from a dead stop to a dead stop and made a crossing stop in eight minutes with that train, which weighed, including the engine, over 500 tons. Now when you have to pick up a load of that kind and run with it and stop again, it requires more power than any electric locomotive could give with the same weight. Talk about the weight on the drivers, that engine had all the weight on the drivers she could utilize, and she gave a drawbar pull of 20,000 pounds. An ordinary train does not require to pull it after it is started more than a 1,000 pounds to the car, so that 20,000 pounds adhesion is all any engine can utilize. She would slip her drivers in starting. But by carefully starting, she has all the adhesion she can utilize. That engine pulled up an 86 foot grade from St. Paul to Minneapolis, a train of 14 cars that weighed on an average of 85,000 pounds a car. The engine itself and tender weighed 100 tons. Now that was a drawbar pull of 23,500 pounds. I took indicator cards, figured the work done on the drawbar and it was 23,500 pounds pull on the drawbar, which was about one-fourth the weight on the drivers—a little more than one-fourth the weight.

Now as to the question of efficiency. Our friend here has stated that only 45 per cent. efficiency is obtained in regular work. We made a large number of tests extending over a month on the Lehigh Valley road, where we had opportunities, with a dynamometer car, and knowing the grade, to figure the lifting of the engine—lifting a train a certain distance into the air in a given time—where we got a horse-power for 20 pounds of water, by the pull on the drawbar, 20 pounds of water in a horse-power hour. The work done and the efficiency, as far as the friction of the engine—the difference between the friction of the engine and the indicated horse-power would not amount to more than ten per cent. in that work, at a speed of about 30 miles an hour. Now these are only pointers. I have not taken time to go into an elaborate paper. I simply give you a few facts which I have dug out by hard knocks right on the locomotive myself. Now as regards the question of speed and also the question of the even pressure on the rail, I have lately built a locomotive which is so perfectly balanced that at a speed of 75 miles an hour you can read a newspaper in the cab. It rides as easily as a Pullman car, and the pressure is absolutely the same at every part of the revolution. That engine will do its work with the lowest grade of coal that you can buy in the market; it makes no smoke, throws no cinders and makes no sound from the exhaust. Now it meets all the conditions, all the requirements that an electric locomotive could meet. The only advantage that an electric locomotive would have over the ordinary locomotive would be that it would give an even pull on the drawbar, an even pressure on the rail and would not throw any sparks or make any smoke. Now the engine I speak of can be duplicated any number of times for $12,000. She will give 2,000 indicated horse-power. She will take 12 Pullman cars from Philadelphia to Jersey City in 90 minutes. The same engine has taken from Susquehanna to Hornellsville an eight car train, with eight stops and five slow-downs, at an average of 61½ miles an hour for the whole distance. When you undertake to beat that kind of work with an electric locomotive, you have got a big contract.

MR. A. E KENNELLY:—Mr. Chairman and Gentlemen: This question of applying electric traction to steam roads has been a fascinating subject to many of us for quite a few years, and I do not think the problem has changed very much in its character during the last five or six years, although its limitations become perhaps more clearly visible from year to year. The manifest success which has attended the introduction of electric street cars where comparatively low speeds are necessary, where frequent stops can be made very cheaply, where a car will stop at any person's door or at any street corner, has been so marked that it has been predicted that the electric locomotive would rapidly transplant and displace the steam locomotive. The conditions are,

however in this case so widely different, that I do not think we can postulate, at first sight, any such conclusion. So soon as you get upon a reserved track, and have to make a high speed, you cannot stop—you have not the time to stop—it costs too much to stop; then you meet the conditions which are found in existing long distance steam roads to-day. For many years this problem of carrying passengers rapidly and comfortably over great distances has been dealt with by steam locomotive engineers, and the maximum economy has been long striven for, that existing conditions will permit. It is contended that if we could replace the steam locomotives of the present time by electric locomotives that we could economize—that we would show an economy by reason of the saving in fuel. Now I never saw any engine that it was claimed would produce a horse-power on say two pounds of coal per indicated horse-power. I have always understood that it took five and six. I will assume that for the present time they do take five and a half pounds of coal on an average throughout the country. The fuel expenses of the roads throughout the country are stated in the statistics to vary from 7 per cent. to 13 per cent. of the total operating expenses, and we may take an average of say 10 per cent. of the total operating expenses as covering the cost of the coal and perhaps also the cost of the water supply. Assuming that we had some kind friend who would take a steam railroad and give us an electric locomotive to-morrow for each steam locomotive, having the same power and being worth about the same amount of money, and also providing the plant and conductors whereby that system could be operated, we should be able to throw out all the tenders, and that would be a considerable saving no doubt. We should save 25 tons or so to every train, and that would enable us to haul a larger load of freight and carry perhaps a little higher speed with safety for passenger service. The difference in coal consumed might amount to as much as twelve per cent upon the passenger trains. It might amount to only three or four per cent. on freight trains. But suppose it averaged about six per cent. We should have a saving of fuel of about 6 per cent. simply by displacing the tenders. The steam plants situated in a central station might possibly, let us say, generate an indicated horse power on two pounds of coal—I fancy that it would be safe to allow three, to meet average conditions of variable loads. With three pounds of coal per H. P. hour at the central station for the transmission system we should probably expect to develop a horse power hour at the shafts of the electric locomotive for about five pounds of coal. We should therefore be very little better off than we are at the present time with good steam locomotives. Even if we succeed in bringing the coal consumption down to two pounds per one H. P. hour, we could not expect to develop a H. P. hour at the locomotive shafts for less than $3\frac{1}{3}$ lbs. We should, however have a greatly increased plant, a much larger inspection and much

greater fixed charges, and we conld not so far as I can see, expect under the conditions of railroad traffic of to-day to see any saving effected.

But the question, fortunately for us, does not lie solely in that compass. Economy is not the sole point of advantage in which we can expect to increase electric traction. If economy were the only factor of passenger transportation, we should never find that the passenger steamers which run across the Atlantic would increase their expenses every year by putting more and more horse power on board their ships consuming coal approximately in proportion to the cube of the velocity, in order to carry their passengers over the ocean in a few hours less time. And if it can be demonstrated that an electric locomotive can safely carry passengers at say 100 or 120 miles per hour, I think there is a very large field open for the electric locomotive on the lines of high speed. It may cost more, but it is very probable that people would be ready to spend more for high speed passenger service. The conditions at such speed would be so radically different, the problem presented would be so different, the number of cars per train and the character and style of the cars so different that a new problem is presented, and on those lines I think there is a future for the electric motor. But on the lines of the present steam locomotive and the mere substitution of electricity for steam, I do not see where we can claim an advantage. The schedule time which the steam railroad time tables of the last twenty years show us, do indicate an improvement in express train speed, but by no means a marked improvement. I think that is a tacit admission to the effect that the steam locomotive cannot commercially be made to run on existing railroads at very much higher speeds than that attained to-day. How far that may be due to imperfection in the locomotive as a machine, or how far to imperfections in existing track is a more difficult question. But if, as we believe, an electric motor can be produced, making up in lightness what it lacks in drawbar pull, that will haul light cars at a very high speed, then I think we may safely hope for a great future for the electric passenger long distance railway.

Mr. C. F. Uebelacker:—Aside from the matter of expense —not entirely aside from it, but rather a point which brings the question of electric traction away from the question of expense more or less—is the ultimate capacity of the steam roads for freight traffic. As I have been thinking it over for the last few days it seemed to me of a good deal more importance than we have been realizing. The fact is that three or four years ago— in 1892, when probably we had the heaviest traffic of any time on the steam roads, quite a number of them were operated on the verge of congestion. It was a strain all the time to keep the freight moving, and particularly to keep the fuel which runs our cars here in the East, moving from the West. At that time

some of the gas managers around this portion of the country were looking each day to see where their coal for the next day was. When things get into that condition it has come to a point where we have got to find some method of conveying our energy from the coal fields, if that is where we have got to get it from, to the point where it is consumed, in some more concrete form than by hauling fuel in cars. The actual fact is that on several of our roads the coal and coke carried amount to a little more than fifty per cent. of the total freight tonnage. The removal of a portion of the tonnage would, of course, make room for more manufactured products. I think the feeling amongst all the railroads is that they do not want to spend any more money; they do not want to lay out the enormous investments which would be necessary for them to materially increase their carrying capacity. The terminal facilities particularly are pretty expensive—around the City of New York for instance. I think it is getting narrowed down to a question of what form of transmission we will use in order to get energy from the coal fields (the main sources of power) to the point where manufactures concentrate. We have several before us. We have heard a good deal about compressed air in the last few months. I have been looking it up as carefully as I could, but I cannot find very much data about it. As applied to street railroads, the best I have been able to find was in a letter of Mr. Ira Harris, written to *The Railroad Gazette*, along in October sometime. He concludes that air can be compressed at about six cents per thousand cubic feet of free air, and that it is going to take to run an ordinary street car about 417 cubic feet of free air per mile at the speed we ordinarily run. That looked to me very much like about two and one-half cents a car mile for power, and I do not think we are prepared to take power at that rate just at present. I think a fair average for electric work would be about a cent a mile. That is what it costs our road—a little under perhaps. I think the conclusion so far as compressed air is concerned, would be obvious under those conditions. We have the explosion motor of various kinds and descriptions, but there does not seem to have been enough progress made in that to tell what the outcome would be if it was put in large units. Probably the most promising thing is the gas motor, and the distribution of gas for power purposes through a piping system. The piping system is not a thing that we look on with very much favor as compared to a copper circuit—rather more trouble to keep in shape. If, then, we come down to the question of transmission of the energy to the manufacturing centres from the coal fields by electricity, it would naturally follow that the roads carrying the freight between those centres would adopt that power too, and it would come down more to a question of what variation we would have to make in our present practice to adapt that power to long hauls. The first thing that would strike us is that we could not very well go

at it in the hammer and tongs manner that we do in electric railroading. That is, we cannot put in power enough to start the whole road at once, if necessary. It is altogether too large a problem. The excess of power we would have to provide would run into too big a figure altogether—both the excess capacity of the line and the excess capacity in the power-house. The only way in which we can overcome that at present is by the use of the storage battery. Of course, the nearer we get to the point where the power is actually used. with the storage battery, the more we save in the capacity of the line and the station. Putting the battery on the locomotive itself we save in the capacity of station and line both, and approximate very nearly the ideal line. At the same time we cut down the enormous fluctuations which would result from the use of electric locomotives on a line where no such provision is made. Let us try to imagine once the conditions which would exist without such provision for fluctuations, if we have, say, five or six switching engines in the yard moving around at once, and perhaps the Chicago Limited moving out and getting up speed as fast as possible, and four or five locals pulling out of the train sheds. It would take about a 6,000 horse-power plant, and altogether it would make fluctuations from about ten units that the load of that plant would be depending on. They would be accelerating, shutting off and accelerating again, continually. It strikes me that the ammeter would want to give up its job in disgust; in fact, an attempt to work with electric power under those conditions I think would be out of the question, and it comes down to the question of a suitable storage battery to be used on the locomotive. The storage battery is more or less of a problem in itself, but the experiments up on the Third Avenue Elevated, I think, will demonstrate what can be done in that direction. The plan they are adopting there, is I believe, that of placing the storage battery on the locomotive, and that, I think, will be the solution when we come to require a solution of the question.

MR. ELIAS E. RIES:—I have listened with a great deal of interest to the discussion this evening. I came too late to hear the first part of Dr. Emery's paper, but it seems to me that this whole question of Electric Traction Under Steam Railroad Conditions might be resolved into three divisions and be considered under three distinct and separate heads. First, we must consider the general question of feasibility, practicability, and economy, in substituting electric locomotives for steam locomotives as such. That point I think has been already demonstrated.

We know that electric locomotives of very large size and power are practicable. The best evidence of that fact is that there are to-day in Baltimore electric locomotives of 95 tons weight which are hauling the heaviest kind of steam railroad trains, including the steam locomotives attached thereto, through the B. & O. R. R.

tunnel, day after day. They are, in fact, developing an amount of tractive power of which steam locomotives of the same weight are incapable; so that the question of *feasibility* of electric locomotion under even the severest steam railway conditions is definitely assured.

The question as to whether or not the substitution of electricity for steam is at present *economical* depends entirely upon the length and character of the railway, its location and upon the nature of its traffic. We must accordingly, before we give an answer to this question, classify these steam railroads, and proceed to consider their operating conditions under the two additional heads alluded to, the first relating to such conditions as are found, for example, on comparatively short interurban railroads, or still better, on the New York elevated railways, where the trains move at close headway, where they are comparatively light, where the structure itself is reasonably short—being less than 10 miles in length—and where all the conditions approximate that ideal which electricians look upon as the best and most economical for a railway service in which the motive power is derived from fixed stations; that is to say, a system in which the train units are short, frequent, light, close together, and in brief, a system in which the load is subdivided and, as nearly as practicable, distributed evenly and smoothly over the entire line. That condition, I think, is fulfilled very closely by the elevated railway systems of this city, and I believe all things considered, that the substitution of electricity for steam locomotives ought to commence right here under such conditions as those. I am of the decided opinion that the arguments against the substitution of electric locomotion for steam on ordinary trunk lines, which have been brought out to-night by our steam railroad friends, would not apply to conditions of the kind that are met with on roads like the elevated or on any reasonably short interurban passenger railroad.

Coming now to the third division or heading, which relates to and includes the long distance or trunk line roads, I may say that we are not at present, *generally speaking*, in a condition to compete commercially with the steam locomotive. Although speculations as to the ultimate use of electricity on long distance steam railroads have been quite frequent, these have never as yet amounted to anything in a tangible way. The steam locomotive is admittedly quite inefficient as a power producer, using, as has been said to-night, from five to six pounds of coal per horse power developed, whereas by generating the power at properly equipped central stations, using large, efficient stationary dynamos, and evenly loaded stationary compound-condensing-engines and all the modern coal and labor saving appliances, the energy can be produced at two or a fraction over two pounds of coal per horse power, besides effecting a considerable saving in labor. The margin of saving in favor of the stationary plant is too great

to be ignored, where it can be turned to useful account. Yet there are other features which render it impracticable, *under present traffic conditions*, to operate, electrically, even moderately long distance steam railways. In the first place the trains, instead of being reasonably frequent, close and light, are few, far between and excessively heavy. It has always been the aim of steam railroad managers, and not without reason, to crowd as much traffic as possible into as few trains as could be consistently run, with due deference to the cost of additional train service on the one hand and the clamor of the traveling and shipping public for increased facilities on the other, and so long as these conditions last and the trains are made heavy enough and the schedules separated enough, the steam locomotive can undoubtedly do the work more economically. However, I do not believe that the limit of substitution of electricity for steam will have been reached when we equip a system like the elevated roads or like a short surface road connecting two populous cities or towns and doing a very large and frequent local passenger traffic; but am firmly of the opinion that, with the development of higher transmitting electromotive forces and more simple methods of conversion than those which are now practicable to use, we shall be in a position to attack the problem of general electric locomotion on trunk line roads. This will be the case more especially when we have learned how to build our road-beds and track, so as to permit of higher speeds with safety than are possible with steam locomotives. An electrically welded track, free from mechanical joints of the present type, will go far to bring about this condition of perfection and at the same time diminish the transmission losses due to artificial track-bonding that now enter so largely into our calculations on line losses and copper cost. It is not so much a question of cost as of ultimate construction and operating conditions on these roads. The mere question of economy in coal, as has been fully pointed out this evening, does not enter so largely into the results to be obtained as to be the controlling factor. The consumption of coal alone is only a small fraction of the operating expenses of a road at best, and while saving in that respect is very desirable, yet the absence of it would not keep back the introduction of electricity, if electricity can show decided advantages over the present method of operating by steam, either in speed, safety, frequency of train service, cleanliness, lessened proportion of dead load of locomotive and train to passengers, improved methods of lighting, heating and braking, enhanced facilities for handling and dispatching trains, general attractiveness to passengers, or in any other way.

Mr. Kennelly has well said that the transatlantic steamship companies have found high speeds to pay even at the expense of the well known enormously disproportional increase in coal consumption. We can readily save *one-half* of the total coal consumed per hour by our modern transatlantic liners by running

at a speed only a few knots less; or, if we desire to economize still further, we can dispense with coal altogether and cross the ocean under sail, as our grandfathers did before us!

If electric energy generated at power stations is ever to be applied to steam railroad work for long distances, and of this I have no doubt whatever, it is, I believe, necessary that such a sytems shall use high tension alternating transmitting currents, converted at points along the line of way into safe lower-tension operating currents, either alternating or direct, preferably the former, and fed to the locomotives in this converted or lower-tension form, as I stated in the course of my remarks at the last meeting of the INSTITUTE. The introduction of electricity upon steam railroads will bring about new traffic, operating and economic conditions that are now scarcely dreamed of, and upon these conditions its superiority and commercial success will depend.

There is, to my mind, no form of energy yet discovered that is so safe, flexible and economical and so well adapted for purposes of transmission as electricity, whether it be used for railway or other work. It has, like any other method of power transmission, its limitations; but these limitations are by no means as restricted as in the case of other well known transmission methods with which we are familiar. Nor is there, so far as I am aware, any available form of *stored or portable* energy that is at all comparable, weight for weight or bulk for bulk, with the mechanical energy latent in coal, which latter is now used as the source of motive power on steam locomotives, and we cannot accordingly hope for much improvement in this respect. This coal constitutes, with the necessary water, tanks and converting plant not directly required for tractive weight, a very large percentage of the dead load to be hauled over the entire road, and contributes largely to the expense of starting, running and stopping the train. If, therefore, any advocate of portable or stored energy in the shape of coal, gas or petroleum, or some peculiar form of chemical energy, or any other primary generating agent, or the discoverer of more efficient methods of converting the stored energy of coal into mechanical energy, were to come to me with claims as to any special merits or utility it may have over existing methods as a source of power for railway locomotion, I should, in all probability, advise him, if the conditions were at all favorable, to convert his power into the form of electrical energy at a fixed station and transmit it over a small and flexible wire to the cars in the form of current, rather than attempt to carry and transform it into motive power upon the train. If the amount of traffic on any given steam road can be made sufficient to justify the entire equipment of a long distance line electrically, the transmission distance can be easily confined within its economical limits by a proper location of generating stations. In the meantime, there is no reason why portions of the lines on which a reasonably continuous passenger service is

handled or can be developed, should not be electrically equipped, subsequently extending the electric service to other trains and thus gradually displacing the steam locomotives. What we need above all for trunk line roads is a sufficiently high transmitting electromotive force and the most simple and efficient type of converters and motors. This much provided, the remainder will come of itself.

I will now return again to the conditions as they exist upon the New York elevated railway system. There we find that the location is excellent for the economical generation of power near the water front, which extends practically parallel along the whole length of the line and but a short distance from it, and it would be a simple matter to put up two or more generating stations for supplying the line. That condition is better than would exist in the case of most long distance steam roads where facilities for the economical generation of power could not be so readily had. In addition we have the advantage of the short length of transmission, which renders it unnecessary to use alternating or converted currents of any type. With a proper proportioning of the stations, it seems to me that a direct current which is fed to the working conductor at a net electromotive force not greater than 600 volts, would do the work; and in that connection it seems to me, after carefully considering the situation and conditions, that in order to get the best and most economical results on a road such as the New York elevated railway system, it would be desirable to supplement the direct line distribution by means of secondary batteries of comparatively moderate capacity carried permanently on the locomotives of the trains. At a meeting of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, held as long ago as October 9, 1888, at which meeting I had the honor of being called upon to open the discussion, I had the pleasure of describing briefly a combined system of this kind for the first time, which events since then have proven to be founded upon the correct principles.

This auxiliary battery is preferably to be carried on the motor car, which is of the regulation passenger length and may be used as a smoking car, and consists of a number of sections which are capable of being connected or grouped in series or parallel by means of a simple controller switch. The battery sections are normally grouped in series with each other but connected in parallel with the motors and across the line, when the motors are being supplied with propelling current from the line, so as to produce a uniform load upon the power station and take care of the traffic at congested points or points that are likely to be congested in the normal operation of the road, by maintaining the working potential constant at distant portions of the line that may be subjected to a temporary overload, and by supplying automatically any sudden increased demand made by the motors. These batteries, however, would not be used in that sense entirely, but their importance is far more wide-reaching, as we shall presently see.

There is a great deal of energy lost on the elevated railway system, for example, in descending grades and in coming to a stop at stations, the stops being exceedingly frequent. The grades there are much steeper and more numerous, and the requirements of rapid transit demand more rapid acceleration with quick but gradual stops, than in the case of the steam railroads.

Now then, by dispensing with the steam locomotive and substituting a locomotive which we will say consists of one motor car for the train, the motor car may take the place of the present leading car so as to avoid increasing the length of the station platforms. This motor-car we will assume is provided with four motors placed below the car floor and each geared to its own axle so as to give a distributed tractive effect due to eight driving wheels, the force of which is, of course, supplemented by the weight of the auxiliary battery, which may be readily placed under the seats along the side of the car or carried upon the truck.

The total number of battery cells to each train would be slightly over one-half of the number of volts on the line, and the cells we will assume are divided into four groups or sections, and capable of being connected in series or parallel by means of a special controlling switch used in the ordinary operation of the train. Now then, when a train is descending a grade or coming to a stop at stations, and the current is therefore no longer needed for its propulsion, the connection between the line and the locomotive is interrupted by moving the controller switch, which operation converts the motors into braking generators and also connects the battery sections in multiple series or parallel with the motor circuit in such a way as to cause the battery to oppose *a low counter-electromotive force* to the charging current, thereby enabling the mechanical energy or momentum of the train to be converted into electrical energy almost to the point of stopping, which energy is absorbed by the battery to be given out again by it in the form of useful power whenever required. It has been estimated, that of the total amount of energy developed by the steam locomotives on the Third Avenue elevated railway, 59 per cent. of the power expended on a round trip is used in starting, 24 per cent. in lifting, and only 17 per cent. in traction. That means that 83 per cent. of the total power expended upon the elevated railway system in this city is consumed on account of stoppages and grades, of which nothing is recovered, but on the contrary, the waste is augmented by the further use of steam to apply the brakes. If we can reclaim only one-half, or 50 per cent., of this wasted energy by the method described, which I believe by this plan is entirely within the range of practice, it would produce a very great saving in power that could not be accomplished in any other way, and in this manner we would get the benefit, by very simple and reliable means, of the most economic method of operating these roads that to my mind, can possibly be conceived of.

Although the cells of the battery are just sufficient in number to develop the normal line electro-motive force when the sections are connected in series, their size or current-capacity, and consequently their weight, as compared with an ordinary secondary battery car equipment, need be but moderate because of the almost constant accession of charging current and the comparatively limited supply of stored power which this system would require, whether for the maintenance of a uniform station load and equalization of working potential over the entire system, or for emergency use. The storage battery to-day is a far more perfect and reliable affair than it was eight or ten years ago, and as cells are now made that are capable of standing a heavy emergency discharge without injury, it is entirely practicable to equip the motor-car with reasonably light weight cells having a capacity sufficient, if need be, to not only permit the train to make a complete round trip under full load without any aid whatever from the power stations, so as to keep the line going in case of any accident or temporary shut down at the power house, but also to take *entire* care of its own train for shorter periods on certain sections of the line during the hours of heaviest traffic. The battery in such case will have been previously charged during the hours of light travel, while grouped in series and connected across the line in parallel with the motors during the usual operation of the train, or, in the case of "extra" trains, during the time the latter are at rest. This charging process just referred to, relates entirely to the "main" charging in cases where it is desirable to run certain trains during busy hours entirely or almost entirely by their own battery power, as on branches or portions of the line remote from the power station, or on the middle express service tracks (which latter in the case under consideration, it might be desirable to leave unequipped with supply conductors because of the frequent occurence of switches, crossovers, and turnouts,) on which few or no stops are made after the train is once under way. In fact, all batteries intended for such service would receive their "main" charge during the intervals of light travel, while these as well as all the other batteries on the line would receive their normal or "maintenance" charge automatically, according to circumstances, from the line or from the motors when the latter are acting as braking generators as I have already described. Any overcharge of the battery occurring under the last named condition, such as might take place on a long down grade, during which the acquired speed of the train is checked or maintained constant by the conversion of its surplus energy into stored electrical power, is of an advantage in that it is practically given back to the line when the battery is again connected in series, which battery will for the time being act much in the same manner as a booster to the line and its feeders until it has again reached its normal charged condition. In other words, the batteries when arranged as I have pointed out, will not

only conserve the energy that is now so largely wasted in the operation of the elevated railways by steam or even by any ordinary direct electrical method, but, by continually and automatically giving and taking energy, they ensure a steady and uniform electrical pressure over the whole line, and relieve the machinery at the power station from injurious, unequal and fluctuating loads, permitting of a comparatively small installation that will generate the *average* load continuously under the most efficient and the best possible conditions, a desideratum the importance of which was fully and ably dwelt upon in the inaugural address of Dr. Duncan, delivered before this INSTITUTE last month. For the same reasons, as was also pointed out, the cost of feed wires can be very considerably reduced since they are called upon to carry only the average load, or I may say *less* than the average load, since any sudden or constant increase of current that may be needed is instantly supplied by the battery of the train *at the very point where it is needed* and without any additional loss in transmission, and since a large part, probably one-third, of the total energy required in the operation of the system *is produced and generated upon the train itself.*

The employment of an auxiliary secondary battery upon a car or train in combination with a line conductor, of which I believe I was the originator, having taken out basic patents on this combination and method of operation as far back as 1886, has many important advantages in addition to those mentioned, that can only be fully appreciated when it is given a thorough trial on a road such as the one under discussion, where the conditions are particularly favorable to a combined system of this character. Not the least of the advantages is that aside from its regulating, governing and conserving qualities, the battery can be called upon in cases of necessity for a very considerable amount of reserve energy of its own, independent of any aid from the power station or line, a fact that will be appreciated by the management of a railway system which is almost the sole dependence for transit of over half a million passengers daily, the bulk of whom require to be carried in the course of only a few hours. As a matter of fact, all the advantages of both the secondary battery and line conductor systems of operation are obtained by this combined method, without the defects of either system taken by itself. The first cost of the necessary batteries will be less than the saving effected by them in station plant and feeders alone, while the economy in operation will be enormously increased. The battery of each train will be available for the purpose of electrically lighting and heating the cars independently of continued connection with the line, fluctuation in voltage being prevented by permanently connecting and supplying the lighting, heating and other train-service circuits from the separate individual groups of cells so that the change from series to parallel connection of the battery sections will have practically no effect upon the normal

voltage of these circuits. By this arrangement also, a lower and more desirable voltage may be used for supplying the lamps, heaters, etc., than in cases where they have to be placed in series directly across the line. Even the motors themselves may be operated, should occasion require, in parallel or series-parallel from the batteries. As the battery is a fixture upon the train, it does not require removal or handling, and the expensive duplication of cells for charging purposes required in ordinary secondary battery car work is entirely done away with. Furthermore, owing to the fact that the cells are almost continually kept up to their normal charged condition by accession of current from the line and motors, the current capacity of the cells that *are* employed can be correspondingly diminished, so that the total cost on account of battery equipment can be vastly reduced as compared with that of an independent battery installation in which a six to eight hours' current supply must be provided for. It is needless for me to add that such a system as I have described is to-day not only quite practicable for roads like the New York elevated, but that it would wonderfully enhance and improve the service and facilitate the handling, dispatching and running of trains, and this at a very material and almost incredible saving over the existing steam locomotive system.

Mr. F. W. Darlington :—In connection with transmission on electric roads, it seems to me that two classes of problems have to be solved – ordinary trolley systems operating single light cars—large trolley systems such as the New York elevated, operating trains with heavier cars—and the regular steam railroads. The difference between the elevated roads and the steam railroads is simply that in the case of the elevated road, it is a large trolley road in that it has frequent stops; though not so frequent as on the surface, but still the problem in a *measure* is the same. It seems to me that the solution under experiment on the Third Avenue elevated road with storage batteries will not produce any results directly applicable to the other problems. On the elevated road the trains are running under momentum part of the time, and part of the time they are accelerating their speed as rapidly as possible. For the conditions existing on the elevated roads it may be possible to charge the storage battery sufficiently during the time of running with momentum, to supply the train with all that it will demand at other times. When you come to steam railroad conditions, however, you have the condition for express trains; that power is put on and stays on during an entire run over a section. Take, for example, the "New York and Chicago Limited" over the Pennsylvania Railroad; it runs from Philadelphia to Harrisburg without a stop. There they change engines and make another long run, and during the time it is running, if it is supplied from a central power station electrically, the power taken will be practically the same during the whole run. This we have demonstrated on the Burlington and Mt.

Holly branch of the Pennsylvania Railroad Company in New Jersey. They have considerable grades there. We found that the indicator on the ampere meter at starting the train would jump to a certain point and fall gradually while the train makes a run over the entire road (in proportion to the speed at first)—but after it gets speed, its maximum speed, the indicator will remain practically constant independent of the grades. This is because the speed is varied on the grades, the train going slower up hill than down, but calling for an even supply of power To such a problem as this it seems to me the storage battery (while at first thought I was struck with the beauty of the solution proposed for the elevated road) will be of no use however, and one other thing in connection with the work over there that applies to the problem of changing over from steam to electricity is the ability to get up speed rapidly. The road there is 7¾ miles long; the last three-quarters of a mile of it are so-called "yard limits," counting from the Burlington end. The distances are all laid off by mile posts. Starting out from Burlington, in six miles we have obtained a speed of 72 miles an hour and maintained it for three miles, and slowed up again so as to have the train under control by the time we reached the yard limits, six miles and a quarter. I am confident that no locomotive on that division of the Pennsylvania Railroad can do that with an ordinary light train and on those grades. Another time we started on a two per cent. grade with a train of cars making the total train load two and a half times what the motors were calculated for, at a point three-quarters of a mile from a mile post which is located at the top of the grade and then practically level. We made that first mile, from mile post to mile post, at the rate of 48 miles an hour. With these results and the ability of motors to start a train quickly, it appears to me that this problem of the substitution of the electric motor for the steam locomotive is not so far off as it has been pictured to-night. Of course, the whole problem, so far as I can see, resolves itself into the method of transmission of power from the generator to the line. This point has not been solved yet. It of course includes the method of collecting the current. The third rail seems to be a good thing if it could be used and be safe, but we have made that 72 miles an hour with an ordinary overhead trolley wire with very little trouble. I am very much interested in the solution of the problems that are under way at the present time, and I think that line being built between Baltimore and Washington is going to give us some very practical results in the method of long distance work. In the matter of grades—we must remember, in first attempting steam railroad work, that our conditions for high speed work are practically the same as for steam railroads. In laying out new roads, grades and sharp curves must be eliminated if we want to obtain high speed.

[Adjourned.]

DISCUSSION CONTINUED IN CHICAGO, OCTOBER 28th.

(Mr. F. E. Drake in the Chair.)

MR. H. M. BRINCKERHOFF:—In a general way it may be stated, that the only methods of electric traction that have so far demonstrated their ability to perform such work as would be required on surface steam railroads are some modification of the much abused trolley system. This may take the form of an overhead contact or of a third rail; but the principle of a continuous conductor fed from a central station with a direct current of from 500 to 700 volts, is common to all.

We have an example of the overhead arrangement on a large scale in the Baltimore and Ohio tunnel equipment at Baltimore. There they are hauling probably the heaviest train units ever successfully handled electrically, and are in fact doing the work of the largest steam locomotives, under strictly steam railway conditions. This, of course, does not allow of the best economy being obtained, but it demonstrates the ability of electric motors to do even this extremely heavy class of work.

In order to supply the large amount of current required on this system, the overhead contact arrangement used is necessarily heavy, and hence expensive. The cost of this particular form must be so high as to make it unavailable for lines of any great length.

The N. Y., N. H. and H. R. R., on their Nantasket Beach line, have successfully operated an overhead system, the trolley wire used being of a pear or figure 8 cross-section, the same as that used on Clark Street, in this city.

The difficulty experienced here was, I believe, in keeping the trolley on at high speeds, and that this, with other considerations, has proved a drawback is emphasized by the fact that this road is now experimenting with a third rail system.

Other companies might be mentioned which are running lines with a few cars as they say "experimentally." These, however, are sufficient to show that a genuine effort is being made by steam railway managers to get at the facts as to whether there is any economy to be gained by the substitution of electricity for steam on some of their lines.

In elevated railway work we have the Metropolitan elevated railroad operating a third rail system on about 14 miles of double track and handling 1,200 trains per day.

The Lake Street elevated has been changed from a steam to an electric road, using the same equipment as the Metropolitan.

The North Western elevated railroad, now building on the north side of the city, is to be equipped electrically, and the "Alley L," now operating by steam, will in all probability follow suit. Thus we will have in the near future the whole elevated railroad system of Chicago run on a uniform third rail plan, giving the finest service of the kind in the world.

I have read some accounts of an experiment now being tried on the 34th Street branch line of the Manhattan elevated railroad in New York, in which storage batteries are carried on the cars to act as an auxiliary to a third rail. The object is, I believe, to have the batteries help to maintain the voltage toward the end of the line, haul the cars in case of failure of the main power station, and also reduce the sudden loads on the generating machinery. This strikes me as an unnecessary complication, and I do not see why the advantage claimed could not be gained from a stationary battery suitably located.

Having now run hastily over the various systems that are at present in operation, and using them as a basis for our judgment, let us consider what changes or modifications should be made in them to meet the requirements of surface work.

In the first place, in proposing to install an overhead system we are met by the following objections:

1. The danger to employes and passengers caused by poles between the tracks.
2. The expense of an overhead construction.
3. The difficulty of maintaining contact at high speeds.

In the matter of danger to employes, the fact is that in yards where there is often a complicated set of switches, the presence of poles placed at irregular intervals would be a great menace to trainmen and switchmen in making up trains. Frequently having to do the work at night, and being obliged to jump on and off the cars at all times, collisions with the poles in these yards would be almost unavoidable.

I have talked with yardmen, switchmen and trackmen, who work about some of the large railway yards in this city, and they all seem to feel that the presence of poles would be a source of danger to them in doing their work.

As to the expense of an overhead construction, the difficulty arises in supplying the large starting currents required in this class of service. Not only must the conductor have a large cross-sectional area, but the surface contact with the device on the car must be large to prevent burning at the points of contact. This of course means a heavy pole construction which runs up the first cost.

The difficulty of maintaining contact at high speeds, particularly where crossings must be made, is one which will have to be overcome by some radical departure from present methods, such as the trolley wheel and pole.

Some of the objections to a third rail system in surface lines are as follows:

1. Danger to employes, particularly in yards, from the bare rail.
2. Danger to public at grade crossings.
3. Danger to passengers where platforms are used on a level with the track.

1. In judging of the danger to employes, I think we can get a good basis for an opinion from the experience on the Metropolitan elevated, which has been operating a third rail system for more than a year and a half. The work we do in the yards is practically the same as that required on surface lines; switching and making up trains, etc., much of this having to be done quickly and at night. In addition we do such light repair work as is required on the trucks and brakes—removing and replacing brake shoes, often with the bare contact rails within a few inches of the work.

I have looked over our accident reports, which are very complete and include every mishap, however trifling, and in the time we have been in operation we have had scarcely any men burned or injured by the trolley rail in the yards, and such accidents as have occured have been of a trivial nature.

The danger to the public at the grade crossings could be eliminated by omitting the contact rail at such points, and providing for contact by placing a set of shoes on the passenger coaches, and carrying a sufficiently heavy wire through the train to supply the motors as well as the heaters and lights.

In this way such crossings as we have in Chicago which range from 66 to 80 or 100 feet in width could be spanned without losing contact. Additional safety could be gained by having a section of the contact rails on either side of the crossing normally out of circuit, and thrown in automatically either by the approaching train or by the guard gates.

3. The danger to passengers could be best eliminated by elevating the platforms which would have the additional advantage of shortening the length of stops, and materially improving the service. With our elevated platforms, passengers have no occasion to go upon the tracks, and those who do venture, seem to take very great care to avoid the "deadly trolley."

In a general way I do not see that there are any insurmountable obstacles in the way of installing an electric system on surface steam railroads following either the line of an overhead trolley or third rail system.

With such experience as we now have, by giving proper consideration to details and the requirements of the service, we should be able to design a system that would give very satisfactory results.

The question whether it should be a third rail, overhead trolley or storage battery is simply a matter to be decided by the conditions on the particular system considered, and a careful estimate of first cost and maintenance.

It is hardly necessary to state that to get the maximum economy from an electric installation it would be necessary to change the whole method of handling trains on steam roads by breaking up the train units. This hauling of a large number of smaller trains at more frequent intervals, limits us at present to systems having

a heavy suburban traffic where such a change would improve the service, besides accomplishing a gain in the economy of operation.

That a system of electric traction can be so installed to meet the requirements of the heaviest suburban service, I do not for a moment doubt. As to whether the gain in economy over steam will be sufficient to offset the additional interest charges on the cost of equipment will depend upon the local conditions, and the judgment and skill used in equipping to meet those conditions.

I have limited myself in these remarks to such systems as are now in practical use and to such lines of work as our present apparatus seems capable of handling, and leave to those more accustomed to theorizing the wider field of possible future developments.

PROF. STINE:—Have you any data at command with regard to the cost of operating the road?

MR. BRINCKERHOFF:—I am not in a position to give you the exact cost of operating, but it is considerably lower than steam, as is shown by the fact that the various railways about Chicago are going into the third rail system, such as the Lake Street elevated, and other roads that contemplate building.

MR. M. COSTER:—Mr. Brinckerhoff's remarks are very interesting. I will state to you what I think will be the future of the electric locomotive in this country. I think we shall not see, in our time, any electric locomotives applied to cross-country service. They will be used chiefly for suburban service—running light trains at frequent intervals.

The advantages of the alternating current motor are going to solve the problem. With the polyphase motor, we shall be able to carry high tension alternating currents over long distances, and change to very low tension for short distances, and so be able to use the third or fourth rail with great success. Thus we could use a very low potential. I look forward to the time in the near future when the alternating current motor will replace the direct current motor for street railway work.

MR. W. D. BALL:—I would like to ask a question regarding the subject before us to-night.—Is cross-country service, or suburban service, the exact topic under discussion?

PROF. STINE:—The discussion at New York took up the whole territory, not only urban and suburban, but interurban and cross-country. Anywhere that we have traffic operated under steam railway conditions, the discussion covers.

MR. BALL:—I rather agree with the last speaker, Mr. Coster, in both positions that he takes, that we shall not for the present see the cross-country railroads operated by electricity, and also that we shall see some developments from the alternating current motor. The great difficulty has been the trouble in obtaining starting torque, and it is a problem which has been worked on for three or four years. The General Electric Company, last spring, announced that they had solved the problem for street railways.

Their idea was to put in a condenser to counteract self-induction, changing the capacity of the condenser. This was found impracticable, so they put in an extra self-induction, and varied that. I have been unable to get any exact data, but I believe that was the idea.

As to the problem of interurban service, I have come to the conclusion that, according to the present developments, it is impossible for us to hope for anything in the near future. The best thing we can do, perhaps, is to transmit high tension alternating currents over great distances, and then transform down. If we have a perfected polyphase motor and controller we can use 40 to 50 volts on the third rails, or we can use rotary transformers and continuous currents. The latter is not simple and is expensive. It is necessary to have the transformer nearly the size of the original machine, that is within 50 per cent. We see that the transmission systems at present are limited to 50 or 75 miles, at a voltage of 15,000 to 20,000. Now, if we wish to transform this into direct current, we would have to put rotary transformers in every 10 or 15 miles perhaps, and transmit at 500 to 700 volts. Reducing the voltage by a static transformer to 500 volts, and putting in a rotary transformer would give you a continuous current at 700 volts. If we must have a station every hundred miles, and sub-stations every fifteen or twenty miles, it is hardly possible to do it with any degree of economy, as compared with steam.

The only point in favor of electricity in interurban service seems to me to lie in the abating of the smoke nuisance. In that way, it would be a great boon to humanity to put in electricity, but as to the economy of it, at the present development, I do not see how any saving can be worked out, and unless that can be accomplished, the steam railroads will certainly not adopt the system.

For cases like that of the Baltimore tunnel, there is a great field for electricity, as well as for urban surface and elevated roads.

I rather lean to the opinion, that for surface roads the third rail system would be dangerous, especially to the ignorant public, and that we had better stick to the small unit and overhead system until we get some perfected battery system, but for elevated roads it is undoubtedly the best system we have. To sum up, the solution for interurban heavy traffic is not found unless it be in the direct conversion of coal into electricity, with some such scheme as Mr. Jacques's battery.

Mr. J. R. Cravath:—The only place where we can look forward to any immediate invasion of electricity in the steam railroad field is in suburban service. There is where the wedge is entering at present, as we see in the case of the Nantasket Beach road which is practically a suburban, or rather, a summer resort service. In order to get the advantages of electricity to the fullest extent for steam railroad work, the steam road must change its

methods of operating by cutting up its trains. Trains must be short, not only for the purpose of making the operation economical electrically, as previously mentioned here, but also for the purpose of giving the public better service, and more frequent service, especially during the hours of light load, and in that way competing with the street lines. The latter is going to be (or at least ought to be) the very best argument in favor of the use of electricity in the minds of steam road officers who are considering the question. On the suburban service of the Illinois Central, we all know the conditions are probably as favorable to the application of electricity as on any steam road in this city. Electrical engineers are sometimes apt to think of steam road men as too conservative. However, from conversation with them, I think steam railway engineers seem to be very much inclined to meet electrical engineers half way in this matter. They have not, however, found anything when they looked up electrical matters which seemed to fit their case exactly. For example, suppose the Illinois Central road should decide to adopt electricity for suburban traffic, what would we have to offer them that would perform their service in a satisfactory manner? There is the third rail system, but we have not decided yet by experiment exactly the best form of third rail or the best position for it, or the best way of insulating and protecting it, or the best arrangement of it for switches, crossings and other special work. That these things will be perfected soon there is no doubt, but we are not there yet. The overhead trolley for such a system as the Illinois Central is considered out of the question, I believe, by the engineers of that road, and they are probably right, because of the large contact necessary for such heavy currents, the difficulties of high speed at overhead switches and trouble from trolley coming off at high speeds on curves, to say nothing of the trouble in switching at terminals.

I would like to ask Mr. Brinckerhoff what is his opinion as to the best position for the third rail if used on a surface steam road—between the tracks, as on the Liverpool elevated and Nantasket Beach road, or as on the elevated roads in this city.

Mr. Brinckerhoff:—That point was carefully considered before equipping the Intramural and again in connection with the Metropolitan Elevated. The objections to placing the conductor rail between the running rails are as follows:

The clearance between the top of running rails at crossings, and the underside of the motors, brake beams, etc., is so small as to make it difficult to properly support and insulate the contact rail. Any part of the running gear or brake rigging falling down or even sagging, will short circuit your system with this arrangement. Again at crossings, particularly where there are slip switches such as we have in our four track combinations, it would be very difficult to maintain continuous contact. For convenience and safety in coupling and working on the truck brakes, etc., an inside rail is very objectionable.

For the above reasons we have placed our rail on the outside of the track, which allows of sufficient height being taken to put in a substantial and efficient insulating support, and reduce the risk of short circuit from dangling brake chains, etc.

MR. CHAS. L. BROWN:—From first appearances the complications that have arisen on the Nantasket Beach line seem rather formidable. Their presence seems unnecessary on a new line, unless they intend it as a purely experimental one. The third rail appears to be the future system, certainly for elevated roads; and as Mr. Brinckerhoff has shown us, it is in favor for surface roads, on account of the danger being less than with the overhead construction. His method of getting over grade crossings is a good one, although it is dependent upon the length of the trains, and the locomotive or motor car alone would be unable to cross without a trolley. I am firmly of the conviction, however, that means will be found to enter station limits and grade crossings with perfect safety and without the use of the trolley. In regard to the alternating current motor: I noticed that Dr. Duncan recently said that after ten years of searching we are still looking for the successful single phase alternating current motor for railway work. From what we have just heard and from information to be obtained in other quarters, we may look to the polyphase motor with its lower voltages and consequent less danger, for the future development in railway work. In regard to the Illinois Central railroad, perhaps, the main difficulty is in securing a sufficiently firm overhead contact for the work required; but on the whole I should say the conditions were ideal for either system of electric traction. Referring to the question of torque raised by Mr. Ball, there can be no general expression connecting the starting and running torque of direct and alternating current motors. It is a question of design, and as the motor referred to by Mr. Coster will give at starting from five to six times the running torque, so the direct current motor may be so designed as to give any desired relation between the starting and the full load running torque.

THE CHAIRMAN:—(Mr. F. E. Drake:) Some ideas have occurred to me in connection with this proposition, having had some little experience years ago in the steam railway line, although nothing which could be used as a basis of argument or contention to-night. If we suppose a long distance line to be equipped electrically, we have a number of features which must be taken care of. These lie chiefly in the fact that the trains are infrequent and very heavy, and in case we attempt to cut them in two, as is suggested in electric practice, I doubt if the railway companies would consent, although in passenger traffic they might rather allow two trains of four cars each than those now comprising seven, ten and even more cars; but in freight traffic we have the very difficult problem, for which I can see in electricity no immediate or satisfactory solution.

A locomotive running under the maximum speed that is usually attained with a loaded train, requires not less than 1,000 horse-power, perhaps more, so that in transmitting that amount of power. not only to that train but to trains going in the opposite direction, the first cost of equipment and feeders must be very high. For short distance traffic, I think the electrically propelled train is the best any one can imagine. I believe the Illinois Central could be equipped with either the third rail or overhead wire system. The principal objection to the latter is the difficulty of maintaining the contact at high rates of speed. I think that contact might be made so that it would not be a serious objection. Perhaps midway between the present trolley and the "cross-bow" used abroad, we might find a plan for preventing the slippage we have here.

The third rail system has some features which are difficult of getting around. If you divide up your line into sections, or according to the "block system," you must have automatic switches. Here we would meet serious opposition from the locomotive engineers, who are seldom called upon to bridge over difficulties of such a character. With a locomotive they are usually able to get to a station or siding. Under this proposed plan, if a switch fails, with the train midway between blocks, there is a very decided wait—one which would cause much interruption to the service, and one which I believe would raise strong opposition and protest.

The feature in connection with steam road practice is the heavy freight duty required and consequent power consumed, yet for long distances I believe the best way to be through the polyphase system, or some system of alternating current, which could be reduced to a practice which would be applicable to such a road.

Mr. Rugg :—I have given this subject very little special thought. One remark made by Mr. Brinckerhoff, however, about the use of the storage battery on the Manhattan elevated railway of New York, attracted my attention. It seems to me that one great advantage would be in the reduced cost of the copper in the feed wires, because the batteries would take the heavy load which would come on the feed wire in starting the train, and the feeders would be taxed with a very small load continuously. I was interested in the remark made concerning the alternating current motor. We all know that the alternating current motor has had a very bad character, but at the same time it has been striving vigorously to live down its bad reputation, and I think it is succeeding very well. People are beginning to believe in the alternating current motor.

The matter of torque seems to bother a great many persons, even at this late day. The experience of late has been that torque does not enter into the question at all. The alternating gives practically as much torque as a direct current motor and just as ef-

ficiently, so I do not think that we need to be at all afraid of the matter of torque in connection with the alternating current motor, even where the torque required in starting is very great. I have seen polyphase street car motors which would give a static torque of five or six times the full load torque, and that, we must all admit, is sufficient for practical purposes.

For train work, a motor is being used now and working on a train in a very successful manner, starting easily and giving very satisfactory service.

MR. DRAKE:—In your experience what is the highest rate of speed recorded?

MR. BRINCKERHOFF:—The highest speed we have attained in tests have been 33 to 36 miles per hour, with four car trains and two motors. This was with a full standing load. With four motors and the same train we have made 38 and 39 miles per hour. These figures are from Boyer speed recorder curves, and the record showed that the train was still accelerating when the current was shut off for the stop.

The speed that can be obtained is largely a matter of the ratio of the gears and the distance you can run. These same motors with different gear made 70 miles per hour, I am told, on the N. Y., N. H. and H. R. R. Such a gear would give very poor economy in our service, as our runs between stops average only 2100 feet.

PROF. STINE:—Do you find that the third rail pits much in service?

MR. BRINCKERHOFF:—No, unless possibly at stations where there has been arcing due to bad contact when ice had formed from the drip of roof or gutters. Even in these places it is slight, and the bulk of the contact rail shows a bright, highly polished surface.

MR. DRAKE:—The question of operating in steam railway conditions would not, in your opinion, operate badly on the shoe?

MR. BRINCKERHOFF:—I do not see anything in this service that should injure the shoe, unless the higher speed at crossings, when you are obliged to allow it to drop and then ride up on the contact rail again. We have used for this purpose inclines, with a rise of one and one-half inches in five feet, and they act satisfactorily at such speeds as we attain, say 35 miles per hour. At higher speeds a more gradual incline could be used, thus lessening the blow on the shoe.

MR. COSTER:—Both alternating and direct current motors can be made to have any reasonable starting torque. So many people confound the polyphase motor with the synchronous motor. But I want to assure Mr. Ball that we can give him all the starting torque he can use. These motors are now applied with the greatest success to cranes in foundries where the work is very exacting—where the mould has to be let down very carefully, where the crane has to lift a very heavy load and the motor requires a large starting torque.

Mr. Ball:—Are you manufacturers in position to put the polyphase motor in the market for street car work, and if not, what is the difficulty?

Mr. Coster:—In street car work there are so many details, such as the third rail, controllers, etc. However, the motor is there, and the results have been far in excess of our most sanguine expectations. It has excelled the direct current motor in many respects, and I think the alternating current polyphase motors will be the motors of the future.

Prof. Stine:—Do you recall any data from the tests made at Pittsburgh on the two-phase street car motor?

Mr. Coster:—I only know the Westinghouse Electric and Mfg. Co. have made some tests with very satisfactory results. We are not quite prepared to furnish data, but I will say that I was surprised to see them do so well.

Mr. Ball:—Why cannot we put in the double trolley and use the third wire?

Mr. Coster:—This question would be determined by circumstances, the same as with the direct current motor.

Prof. Stine:—I would like to ask Mr. Rugg if he has any data on hand of their tests?

Mr. Rugg:—I have no definite data on hand; except in a general way, they are built for heavy torque, and between five and six times the static torque is obtained, the controlling of them being easily accomplished. As far as efficiency goes, it compares very favorably with the direct current street car motor.

Mr. Ball:—Do you put resistance in series with the armature and get it out afterwards?

Mr. Rugg:—Yes; by means of resistance we get the starting torque.

The Chairman:—We have had an interesting discussion this evening on existing methods of practice here in this country, and in some cases, abroad. For myself, I would invite comparisons or remarks on some of the other proposed systems, notably, the Heilmann machine,—and whether, in the opinion of those present the plan of this inventor could be used practically and with success in this country.

Mr. J. R. Cravath:—I do not like to say very much about it. It is hard for me to see where the advantages of such a machine as the Heilmann locomotive would come in. It carries its own steam power plant, which never seemed to me to be of the best design for the work; it also carries its own electrical plant, and I also question whether that is the best design for the work; it piles this all on one set of trucks, and employs a small army of men to keep it going. Of course there may be some theoretical advantages, but I don't care to say much about it.

Prof. Stine:—The subject under discussion naturally invites attention to the storage battery in relation to the future of electricity in the operation of railways. As the storage battery

situation exists at the present time, such a relation seems entirely out of the question, owing not only to the excessive weight of the storage batteries, but to the rapid deterioration of plates, especially when these are made light enough for traction purposes. Some time since, I had occasion to make what was to me a very interesting calculation with reference to the possibility of the application of the storage battery to heavy traction work. I was led to do this from results of a purely scientific character, and wish to state emphatically at the outset that this calculation has no commercial basis at present.

Some years ago, during an investigation of the storage battery, I was enabled to obtain an actual storage of one horse-power hour in 29 pounds of battery. This was under conditions which were far from ideal. By reducing the weight of the containing vessel, and the dimensions of the battery, and doing away with all unnecessary electrolyte, etc., the same result was indicated from a gross battery weight of 12 pounds per horse-power hour. This is a laboratory possibility, but unfortunately there are some questions of an electrolytic character which prevent its having any commercial application. On this basis a calculation may be made, showing what would be the result of applying such a battery to the operation of say our best developed express service on railways.

We may assume that the average traction weight on drivers on our best express engines is about 80,000 pounds, while the front truck carries a further weight of 40,000 pounds. Such locomotives are usually provided with great water and coal endurance. We may take these figures at 13,000 pounds for coal and about 30,000 pounds for water; this, carried on a tender weighing from 30,000 to 40,000 pounds. A locomotive of this size will, at 50 miles per hour, develop about 1,250 horse-power, and is in service for about 150 miles at a time. Allowing 50 per cent. reserve, this will give four and one-half hours' service, or a total of 5,625 horse-power hours. Counting now 12 pounds of battery per horse-power hour, we have a net weight of battery of 67,500 pounds. This would seem to be just a little under the average weight of the tender and its contents. Taking the weights of the electric locomotives so far built, it would be possible to carry this entire battery weight on the driving axles, making the total weight tractive, unless for reasons of great speed, when a leading truck would be necessary. The total weight of the battery and locomotive would not be practically in excess of the steam locomotive weight mentioned above, (120,000 lbs.) The gain in weight would be that of 80,000 pounds charged up against the tender.

As to the bulk of this battery, it could be brought within the space at its disposal in such a locomotive. In summing up our figures, we see that practically little would be gained by using the storage battery in place of the steam locomotive, and the en-

tire question would hinge on the economy of such a storage battery installation over that of a portable steam plant. The object, however, was to show the possibility rather than the economy.

To return from the somewhat ideal possibilities to the problem that we are actually facing to-day, I do not believe that the discussion is at all ended, when we have considered the relative economies existing between the practical steam plants and electric locomotives supplied from large central stations. There are many other considerations which, it seems to me, will eventually be important factors in deciding to what extent steam railways will adopt electricity. In the first place, on a large number of railways the best handling of trains does not seem to obtain very serious attention. The problem seems to be to get trains through. As a consequence, there is a local congestion, especially of freight traffic. This greatly magnifies the problem of change to electric traction. If the flux of trains could be kept near an average, the problem would be greatly simplified, and better management on the part of train despatchers could accomplish much toward securing this. But if those writers who have written and have been competent to express an opinion are to be trusted, railway operation will, in all probability, undergo important modifications, and it is not too much to believe that the shortening up of the trains, with an increase in the number of train units, will be the tendency. This, of course, will be entirely favorable to electric traction.

In spite of all our carefully matured calculations, bearing on the question of relative economy, should the financial conditions of the country at large greatly improve, these would have less weight than we at present attach to them. The coal item is a comparatively small one on a large and well-managed system, and our assumptions of what would be the character of electric traction, if it were adopted, should be taken with great allowance.

After all, there are questions of desirability which will add weight to those of economy, and experience will suggest modifications from time to time which may put an entirely different aspect on the whole problem. Without being too sanguine, it does seem that if a few engineering points were settled soon, that electric traction would gain a rather rapid hold upon the railroads of the country. At present, electrical engineers themselves are in doubt as to whether polyphase motors have any decided advantage over direct current motors. Then, too, the matter of contact between the conductors and the moving trains has not reached such a stage of development as to inspire confidence in its reliability and permanence. A few of these points once settled, we may look for rather rapid progress.

[Adjourned.]

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, November 18th, 1896.

he 110th meeting of the INSTITUTE was held this date, at 12 st 31st Street, and was called to order by Vice-President nmetz at 8 P. M.

he Secretary announced the election of the following associate nbers by the Executive Committee at its meeting in the rnoon.

Name.	Address.	Endorsed by
MER, EDWIN W.	Electrical Engineer, 46 Second Avenue, Newark, N. J.	W. J. Jenks. Chas. A. Terry. A. E. Kennelly.
, H. P.	Engineer, Wendell and MacDuffie, 813 Havemeyer Bldg, N. Y. City; residence, Washington, D. C.	Max Osterberg. Edw. Caldwell. W. D. Weaver.
c, GEO. W.	Electrical Engineer, Chicago City Railway Co., 2020 State Street, Chicago, Ill.	D. C. Jackson. C. F. Burgess. S. B. Fortenbaugh. B. J. Arnold.
ARTHY, E. D.	Electrical Engineer, The F. P. Little Electric Construction and Supply Co., 135 Seneca St.; residence, 451 14th Street, Buffalo, N. Y.	C. R. Huntley. Henry G. Stott. C. W. Ricker.
AB, MARTIN C.	1729 Madison Avenue, Baltimore, Md.	Louis Duncan. H. S. Hering. H. A. Rowland.
US, THEODORE.	Tester, General Electric Co., Schenectady, N. Y.; residence, 1213 Linden Avenue, Baltimore, Md.	C. P. Steinmetz. H. S. Hering. Ernst J. Berg.
AER, ALEXANDER.	Mechanical, Chemical and Electrical Engineer, The General Ozone and Electric Supply Co., Suerkade 104, The Hague, Holland.	H. Doijer. H. F. R. Hubrecht. R. W. Pope.
LACE, CHAS. F.	Engineer, Stone and Webster, Boston, Mass.; residence, 62 Forest Street, Roxbury, Boston, Mass.	A. M. Schoen. Chas. R. Cross Russell Robb.
ING, ALLEN H.	Electrical Engineer, Riker Electric Motor Co., Brooklyn, N. Y.; residence, Stamford, Conn.	A. L. Riker. T. L. Proctor. W. L. Bliss.

WOODWARD, W. C.	Electrical Engineer, Narragansett Electric Lighting Co.: residence, 21 Arlington Avenue, Providence, R. I.	C. H. Herrick. C. D. Haskins. F. V. Henshaw.
WRIGHT, LOUIS S.	Manager, The Carbondale Traction Company, Carbondale, Penn.	S. G. Flagg, Jr. A. E. Kennelly. E. J. Houston.
YSLAS, CARLOS.	Electrician of Railways in Jalapa, Jalapa, Vera Cruz, Mexico.	C. C. Chesney. H. L. Fridenberg. Wm. Stanley.

Total 12.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners, Oct. 14th, 1896.

NICHOLS, GEORGE P.	Partner, Geo. P. Nichols & Bro., Electrical Engineers and Contractors, Chicago, Ill.
FOSTER, SAMUEL L.	Electrical Engineer, Market Street Railway Co., San Francisco, Cal.
CUSHING, HARRY C., JR.	Electrical Inspector, Fire Underwriters' Tariff Association of New York, 32 Nassau St., New York City.
BALDWIN, BERT L.	Mechanical and Electrical Engineer, The Cincinnati Street Railway Co., Cincinnati, O.

Total 4.

The Vice-President announced that the evening would be devoted to the reading and discussion of a paper by Mr. H. Ward Leonard, entitled "Volts *vs.* Ohms." The apparatus described, had been installed upon the platform, and was shown in operation by the author, with the following preliminary remarks.

MR. LEONARD:—It may be well for me to show the operation of the apparatus. I will say in explanation that the apparatus was not manufactured for this special purpose, and that the motor generator is not exactly of the best form for this use. The windings of the two armature ends are not identical, and the field strengths are not the same. The result, therefore, is not quite as good in many ways as it would be if they were very much more nearly identical. There is quite a difference; the voltage is 70 on one and 120 on the other; but I would say that it was loaned by the Crocker-Wheeler Electric Company, and will answer the purpose sufficiently well to illustrate the performance, so I will show it as well as I can under the existing conditions.

If you will turn to Fig. 4 of the paper "Volts vs. Ohms," I will point out what we have here. I have marked upon the board of the machine here the letter S, which is the shunt-wound end of the transformer; and that marked R is the reversible end of the transformer; and the motor M is connected, as you see. This is the reversing rheostat in the field of the reversible machine, which you will notice is quite large relatively to the size of this particular machine. This apparatus would be no

larger, however, if it were to handle 100 kilowatts instead of one: the dimensions being due to the number of contact buttons more than anything else.

I have here a voltmeter, which is connected across the terminals of M, the motor to be driven. I will first start up the shunt-wound motor, and when running at full speed I will now adjust the rheostat in such a way that the line volts of 125 will be opposed by the 125 volts of the armature of M.

When I close the armature circuit you will see there will be no change in the current flowing, and that by the adjustment of this rheostat I will be able to make the motor armature go from rest to full speed; and not only that, but I can make it go slowly in a backward direction, for the reason that the speed of these two machines must be always equal, and as soon as the voltage of R is higher than that of S, the current will be reversed in this loop 1, 2, 3, 4; and the voltage of R being higher than the voltage of the line, M must run in a reverse direction. You will notice that there will be no change in the current when I close the armature circuit, and there will be no effect upon the armature of the motor.

By manipulating this rheostat you can make it go in either direction. By turning in this direction (illustrating), the motor will run in the direction which is the reverse from its full speed direction. In this case the E. M. F. produced by the reversible machine is higher than that of the line. By this device I can run it backward, but if the motion is to be reversed so as to run backward at full speed, it is better to have a reversing switch upon the motor armature terminals which would be thrown at a time when there is no voltage at the terminals, and then you could go backward at full speed as the current through the armature would be reversed.

Now, there has been considerable talk as to whether or not any practical amount of energy could be restored to the line by this system, and I wish to show that this is done. This ampere meter showing the current from the line, reads both ways from zero. Now while running the motor at full speed in that direction, I instantly reverse the rheostat, thus. You noticed that when I did that, current was restored to the line. The armature reversed, and its retardation and acceleration in the opposite direction was accomplished by making the armature of M generate useful energy which is restored to the line.

A Paper presented at the 110th Meeting of the American Institute of Electrical Engineers, New York, Nov. 18th, 1896. Vice-President Steinmetz in the Chair.

VOLTS VS. OHMS.

Speed Regulation of Electric Motors.

BY H. WARD LEONARD.

The control of the speed of an electric motor from a state of rest to that of full speed is a problem of rapidly growing importance to the electrical engineer. The operation by means of electric motors, of elevators, locomotives, printing presses, traveling cranes, turrets on men-of-war, pumps, ventilating fans, air compressors, horseless vehicles, and many other electric motor applications too numerous to mention in detail, all involve the desirability of operating an electric motor under perfect and economical control at any desired rate from rest to full speed.

The most commonly practiced method of controlling the speed of an electric motor for such applications at present, involves the use of ohmic resistance in the circuit of the motor armature, which resistance is varied to control the speed of the motor.

The use of an ohmic resistance for controlling the speed of an electric motor results necessarily in a waste of energy, and in an unstable control of the speed. The object of this paper is to endeavor to show the advantages arising from the use of a system of motor control having several modifications, but all of which involve the idea of controlling the speed of an electric motor by controlling the E.M.F. generated in its armature circuit, and without using any regulating resistances in that circuit.

I shall consider only the control of a single motor, that is, I shall not refer to the control of several mutually dependent motors by grouping in series and series parallel. I shall also limit the consideration to that of a continuous current motor.

Fig. 1 shows the first and simplest form of the E. M. F. system of motor speed control.

S is an engine or other source of power operating at a practically constant speed.

G is a generator.

M is the motor.

E is a circuit of constant E. M. F. which supplies current for exciting the fields of G and M.

It will be noticed that the fields of both G and M are independent of the E. M. F. and current of their armatures. The field of M is practically constant. The field of G is variable from full strength to zero strength by manipulation of the controlling rheostat C in the field circuit of G. It will also be noticed that there is no rheostat in either the field or armature circuit of the motor M which is to be controlled.

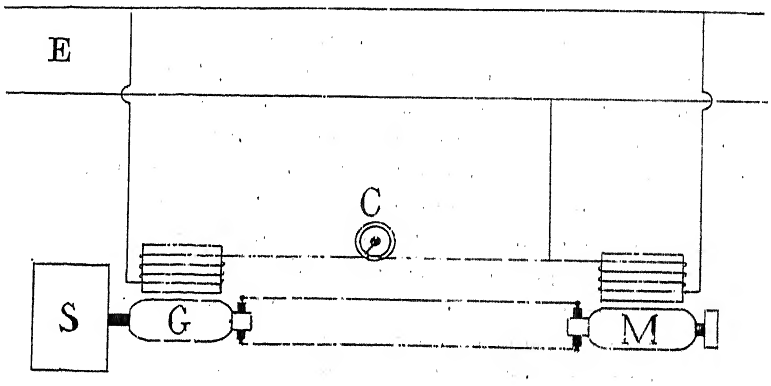


FIG. 1.

It will be evident that by varying the field strength of G we can vary the E. M. F. generated in the armature circuit from zero to the full working E. M. F.

In order to make definite comparisons, let us assume certain figures for the full E. M. F. and current of G. Suppose its full E. M. F. to be 250 volts, and its full working current to be 100 amperes. Also let us assume that the resistance of the armature of M is .05 ohms, giving a C^2R loss in that armature of 2 per cent. of its rated capacity, when the full working current is flowing. Let us assume that the full speed under full torque is 500 revolutions per minute. For the sake of simplicity and because it does not affect the practical accuracy of the deductions, let us neglect the slight losses due to Foucault currents, hysteresis, fric-

tion and the slight ohmic resistance of the rest of the armature circuit.

Suppose that our motor is to drive a large printing press, such, for example, as is used for printing calico, and that it is required of us that we shall drive the press at any desired rate from rest to full speed, and that we shall maintain any such intermediate speed practically constant even though the torque should vary from mere friction torque to the maximum torque of operation.

Let us suppose that the friction torque is represented by 10 amperes through the armature of M, and that the maximum torque of operation is represented by 100 amperes through the armature of M. We have now fixed all the conditions necessary, in order that we may determine the exact performance of the motor.

If, by manipulation of the controller C, we allow a slight and gradually increasing current to flow through the field of G, the E. M. F. at its brushes will gradually rise from zero upward, since the armature of G is being constantly driven at its full speed. When it is generating one volt at its brushes, a current will flow through the armature of G, due to one volt acting through .05 ohms, causing 20 amperes to flow through M. If the press be under full torque it will not start with this current. When we have five volts at brushes of G, we have 100 amperes through M, and the armature is just about to start; but since any motion of M would cause the development of a counter E. M. F. which would reduce the current below 100 amperes, it does not start as yet.

As soon as we raise the E. M. F. at brushes of G above five volts, the armature of M moves at a rate of speed sufficient to develop a counter E. M. F. of five volts less than at G.

Thus if we have six volts at G, the armature of M will move at a rate of speed sufficient to develop one volt counter E. M. F. and permitting the flow of the proper current for the necessary torque, that is 100 amperes.

I call attention to the fact that since the field of the motor is constant, the counter E. M. F. is directly proportional to its speed.

At full torque and full speed, the counter E. M. F. would be 245 volts, five volts being dropped by the passage of the 100 amperes through the ohmic resistance of .05 ohm.

Similarly if G has 125 volts at its brushes, that is one-half of its full voltage, and the full torque current of 100 amperes be in

use, the counter E. M. F. of M would be 120 volts and its speed would be 120÷245 of its full speed, that is M would run at 245 revolutions per minute or practically speaking at one-half of its full speed.

Similarly, with one-tenth of the full E. M. F., that is with 25 volts at the brushes of G, the speed of M under full torque current would be 20÷245 of its full speed, that is 41 revolutions per minute or approximately one-tenth of its full speed.

Suppose now while M is running thus at 41 revolutions per minute under full torque, the entire load be thrown off, except merely the friction the torque current of which we have assumed as 10 amperes.

Instead of five volts drop, due to the 100 amperes through the .05 ohm, we now have only 10 amperes through .05 ohm, or .5 volt drop, and the resulting momentary increase of current through M causes slight acceleration of its armature until its counter E. M. F. is 24.5 volts instead of 20 volts, which it was under full torque. That is, its speed is now under friction load $\frac{24.5}{245} \times 500$, or 50 revolutions per minute.

Hence we see that when operating the motor at one-tenth of its full speed of 500 revolutions per minute, and while under full torque, we can throw off the entire load and experience a change in the speed of only 9 revolutions per minute.

Now let us consider the same motor under same conditions excepting that it is connected as usual to a constant E. M. F. circuit of 250 volts, and that the speed is controlled by an ohmic resistance in the armature circuit, the field being in shunt directly across the line.

If we are to operate the motor under full torque, we must have the full 100 amperes flowing through its armature, and if it is to be operated at one-tenth of its full speed its counter E. M. F. must be 245÷10 or 24.5 volts. This means that we must drop in the rheostat 220.5 volts out of the 250 volts constantly impressed. By having 220.5÷100 = 2.2 ohms in the rheostat we can secure this condition of affairs. But now we are wasting 100 × 220.5 = 22,050 watts in the rheostat and only utilizing in the motor 2,450 watts.

Perhaps the worst feature, however, about the conditions now prevailing, is that we have practically no control over the speed under change of torque. For example, suppose as before that we

now throw off the entire load, leaving only the friction load. Under the reduced torque the motor speeds up until its counter E. M. F. plus the drop of E. M. F. in the rheostat again equals the line E. M. F. When this condition is realized, we have the friction torque current of 10 amperes flowing through the resistance of 2.2 ohms in the rheostat, and causing a drop of only 22. volts, and consequently the counter E. M. F. of M must be 250—22. = 228 volts and its speed must be $\frac{228}{250} \times 500 = 456$ revolutions per minute.

That is, by throwing off the full load, our motor has jumped from 41 revolutions per minute to 456 revolutions per minute,

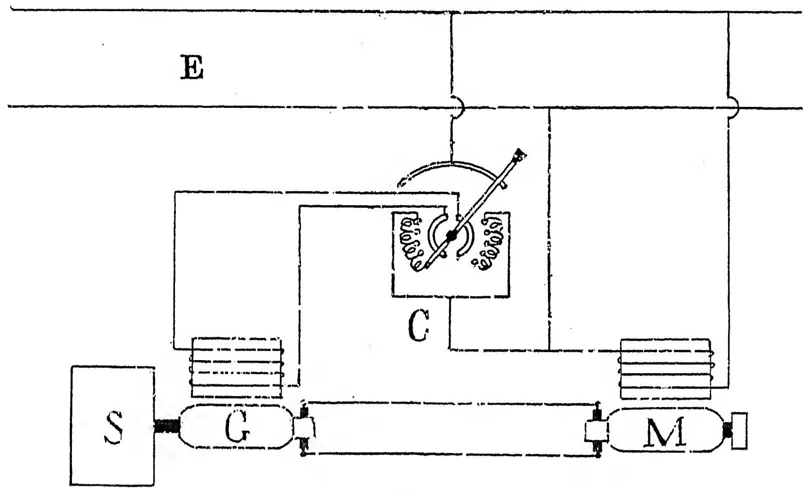


FIG. 2.

a change of 415 revolutions in this case as compared with 9 revolutions in the former case, the change in speed under the same conditions being nearly 50 times as great by the system of ohmic control as by the system of E. M. F. control.

Suppose we are again operating at one-tenth speed under full torque and therefore have 2.2 ohms in our rheostat. Now let the torque increase only 12 per cent. which must be expected in any kind of commercial practice. To keep the armature in rotation will require 112 amperes, but the ohms in circuit, 2.25, will only permit the passage of 111 amperes with 250 volts impressed, hence the increase of 12 per cent. in torque will cause the armature to

come to rest. In the system of control by volts instead of ohms on the other hand, the speed would only be reduced from 41 revolutions per minute to 40 revolutions per minute which change would not be perceptible.

I have gone thus fully into the detailed figures of the cases considered, believing that the radical difference detween the systems of control can only be appreciated fully by such concrete examples as I have given.

I now desire to call attention to the fact that in the speed control by ohms, the operator can, by moving the lever of his rheostat, change the volts upon M as fast as he can move his hand. This is a frequent cause of burning out of armatures. In the case of a reversing rheostat, the instantaneous throwing of the rheostat lever while the motor is at full speed would mean that double the line E. M. F. would be acting to send a current through merely the ohmic resistance of the armature, for the reversal of the rheostat switch would cause the line and motor E. M. F.'s to act in the same instead of counter directions.

When, however, the change in E. M. F. at the motor is due, as in the case of Fig 1, to a change of field magnetism, the instantaneous throwing of the lever of the controller does not result in an instantaneous change of E. M. F. at M; for a change of current through the field of G results in a gradual although sufficiently rapid change of E. M. F. at the brushes of G, and hence the armature of M has a chance to accelerate and develop a counter E. M. F. which in practice will never be greatly different from that impressed. Even an instantaneous reversal of the connections of G can be made in ordinary practice without any detrimental result upon the generator or motor, because of this appreciable time required to reverse the magnetism of G. By various well known methods the time required for this reversal of magnetism can be varied over wide limits.

Fig. 2 shows the changes in connections of Fig 1 necessary for the operation of a motor whose motion is to be reversed.

Fig. 3 shows a modification of the general system in which the source of E. M. F. is composed of several different generators in series with each other and having a system of several conductors, upon each of which a different constant potential is maintained, so that by connecting the motor armature across different conductors, different E. M. F.'s are obtainable at the motor armature. With two generators and three conductors we can obtain three re-

versible, different automatic speeds. Similarly, with three generators and four conductors we can get six reversible, different automatic speeds. This modification of the system is especially suited to the distribution of power in an isolated plant such as a large manufacturing establishment.

I now come to the modification of the E. M. F. system of motor speed control in which the substitution of E. M. F. for ohms in the motor circuit for the purpose of controlling its speed, is most conspicuous.

We found, when considering the case of the rheostat control with the motor running at 1÷10 speed and with 100 amperes

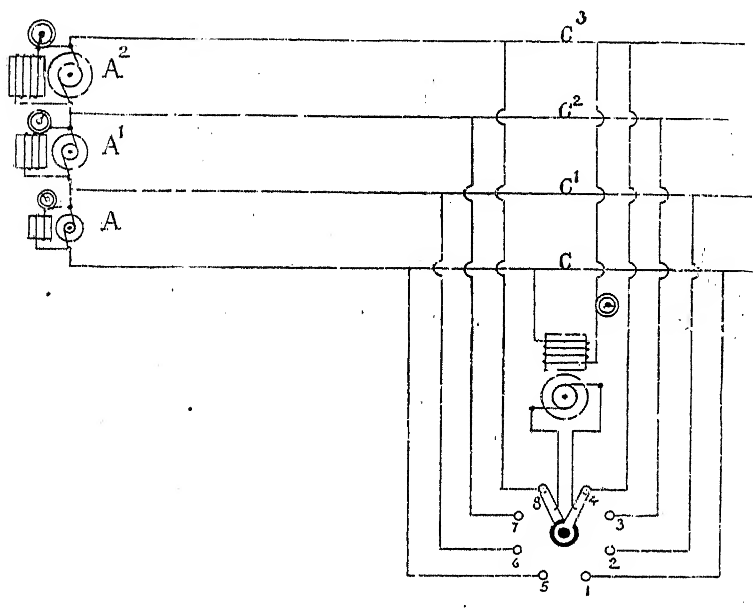


Fig. 3.

through it, that the rheostat had to absorb and dissipate 100 amperes × 220.5 = 22,050 watts while only 2,450 watts were utilized in the motor.

As has been shown, this loss in the rheostat is troublesome, not only because of the waste of energy, but especially because of its interference with all positive control.

Evidently what is needed is to substitute for the rheostat a device which will absorb the 220.5 volts and 100 amperes, and, instead of wasting them, convert them into useful work.

Fig. 4 shows how this is accomplished by one modification of the E. M. F. system of motor speed control.

G is a source of 125 volts constant E. M. F.

S is a shunt-wound dynamo connected across the constant E. M. F. and hence running at a constant speed.

R is a dynamo mechanically connected to drive or be driven by S, and running at a practically constant speed.

The field of R is excited by the main line E. M. F. and is independent of the E. M. F. of its armature and of the current through its armature. It has a variable and reversible field rheostat in

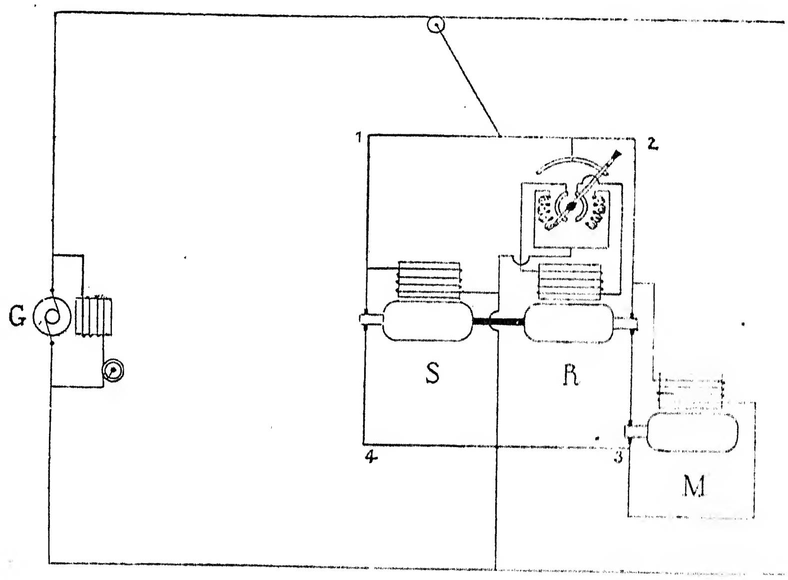


FIG. 4.

circuit by means of which the magnetism of the field of R may be varied and reversed at will.

M is the working motor. Its armature is in series with the armature of R across the line. Its field is excited by the main line E. M. F. and hence is independent of the E. M. F. or current of the armature M.

Let us suppose that the armature of S is wound for 125 volts and 100 amperes. The armature of R for 125 volts and 100 amperes, and the armature of M as in the former illustration for 250 volts and 100 amperes.

I will neglect the armature losses for the sake of simplicity, and because they do not materially affect the conclusion.

Suppose that as before we want to run M at one tenth of full speed under full torque, full speed being 500 revolutions per minute and corresponding to 245 volts counter E. M. F. and full torque being that due to 100 amperes in the full field.

At first let us have the rotary transformer RS so adjusted that the fields of R and of S are both fully excited. Each end takes a slight current through its armature. Both ends are motors, and they divide between them the friction load.

Now let us weaken the field of R until its field strength is only nine-tenths of its full strength. In this weaker field, R tends to run faster; but in doing so it is obliged to drive faster the armature of S, whose counter E. M. F. has been almost equal to that of the line.

The dynamo S now acts as a generator and has two paths open for its current, the first being the circuit through the generator G and the other path being in the closed loop through R and M.

The E. M. F. of G balances that of S, but the E. M. F. of R which formerly was equal to that of G and also that of S, has been reduced by the weakening of its field, hence S sends a large current through the local circuits 1, 2, 3, 4, causing a large torque in the armature of M, in its constant field. M evidently will run at a speed such that its counter E. M. F. plus that of R equals the line E. M. F.

Expressing the conditions in figures under the assumption made, there will be upon the terminals of M, 25 volts and through its armature 100 amperes, that is, a total of 2500 watts in the armature of M. The armature of R will have $125 - 25 = 100$ volts and 100 amperes or 10,000 watts, and the armature of S will have 125 volts and 80 amperes, that is 10,000 watts

The generator G produces 20 amperes at 125 volts or 2,500 watts, which, by the method described, is transformed into 25 volts and 100 amperes at the working motor.

By continuing the weakening of the field of R, we finally have a field of no strength, and hence R becomes inert and we have the full line E. M. F. of 125 volts upon the 250-volt motor M, which, consequently, runs at half speed. Under these conditions no energy is transformed by the rotary transformer RS. If, now, we reverse the connections leading current to the field of R, and send a gradually increasing current around its field, its voltage is

added to that of the line instead of being counter as heretofore, until finally its full voltage of 125 being added in series with the line E. M. F. of 125 volts we have upon M, 250 volts and it runs at its full speed. While R is thus adding to the line volts, it of course is acting as a generator instead of a motor, which it formerly was, and is now driven by S, which acts as a motor instead of a generator.

I call attention to the fact that the current capacity of all three armatures, R, S and M, is equal, but the full E. M. F. of R and S is only half that of M, which means that the K. W. capacity of R and S is each only half that of M.

The rotary transformer R can also be designed to run at much higher speed than is demanded for the working motor, since it can be perfectly balanced, and is free from any side or end thrusts and has a minimum friction.

There are other modifications of the E. M. F. system of motor speed control which I am not able to describe at present, but as in the case of those described above, the underlying feature is, to insert or cut out E. M. F. instead of ohms in the armature circuit of the motor when we wish to change its speed.

Since many have thought this system of motor speed control limited to a few peculiar cases, and also limited to peculiar kinds of generators and motors, I give in the table below, instances within my own knowledge, showing the kind of machinery operated and the size and make of the motor used:

Kind of Machinery Operated.	Size of Motor.	Maker of Motor.
Traveling Cranes.	1 K. W. to 50 K. W.	Crocker-Wheeler, Eddy, Edison, Waddell-Entz, Billberg, C. & C., Westinghouse.
Passenger Elevators.	5 K. W. to 40 K. W.	Edison, Eickemeyer.
Mining Hoists.	10 K. W. to 125 K. W.	C. & C., Crocker-Wheeler.
Turrets on Men-of-War.	25 K. W.	General Electric.
Billet Shifter in Rolling Mill.	30 K. W.	Crocker-Wheeler.
Heilmann Locomotive.	8 of 50 K. W. each.	Brown.
Cloth Printing Press.	25 K. W.	Edison, General Electric.
Newspaper Printing Press.	50 K. W.	Unknown.
Universal Boring Machine.	5 K. W.	Unknown.

Discussion.

Mr. G. S. Dunn :—This method of control is so perfect that it ought to be developed so as to be applicable to most of the problems we have to deal with. What has stood in its way has been high first cost. The method that Mr. Leonard has shown to-night is important because of bringing the first cost of extra or regulating apparatus down one-half. He has indicated how the cost can be still further reduced by running the motor-generator at a high speed, which is permissible because it is not in connection with any other machinery. I desire to point out that even a further reduction of cost can be made if there should be demand sufficiently great to warrant the design of special motor-dynamos.

Where control is needed, the work is usually intermittent. If advantage is taken of this fact, a motor dynamo can be designed small, whose sparking limit of load is much out of proportion to its heating limit. This will permit it to handle the currents demanded without sparking, while, on account of the intermittent character of its work, it will not become too hot. For a given output, machines of this type could be made considerably cheaper than the standard machines which now have to be used. This would bring the method of control very much closer to general usefulness than now, and I do not doubt before very long, such special motor-dynamos will be put on the market.

Dr. C. T. Hutchinson :—I think we will all admit that the method of control exhibited here is very pretty. I am quite sure no motor armature can be controlled quite as nicely by any other method; but, as Mr. Dunn has just suggested, the whole question of its practicability is one of cost—not first cost, to my mind, but cost of operation. I doubt very much if the cost of operation of the apparatus of this kind can be brought down as low as with the more usual method of control. Every time this matter has come up before, I have taken the position that this is the least economical method of operation, and I am still of that opinion. I saw Mr. Leonard's paper a day or two ago, and to get an idea of what was being done I have examined some of the plants in the city running with this general method of control, not precisely similar, but on the same lines as Mr. Leonard's method; and of one case in particular—that of the New York Clearing House plant—where an elevator is running on this plan, I wish to speak. Here there are four small sidewalk elevators used for bullion, and having a direct rheostat control in the armature, and one large passenger elevator with this motor-generator control. To run this plant there is an engine connected directly to a large dynamo of 50 kilowatts supplying merely the power circuits, and not the lights of the building at all. A large part of the time only the one passenger elevator is in use, and to run it there is the engine and large generator driving a motor, which in turn drives a generator with variable

field; this in its turn drives the hoisting motor: that is, a series of five pieces of machinery—one engine and four electrical machines are used for one elevator. When I was in the building nothing was running but the passenger elevator, and the five pieces of machinery were in use to run that one elevator. The generating set has to run whenever the building is open, say from eight o'clock in the morning until six o'clock at night. That is, an engine of 75 H. P. with large generator runs 10 or 12 hours a day, most of the time doing nothing but friction work, but standing ready to supply power to the elevator whenever required. I venture to say from observations that I made, that the ratio of work done on the elevators to the waste caused by the friction of the generator turning all the time is considerably less than one-tenth; the all-day efficiency cannot possibly be 10 per cent. The claim is made for this method of control that the power required in starting the car is very much less than it is with rheostat. Judging from my observation of the same plant, this is not true; it is directly the reverse. The generator was running on a 230-volt circuit; in starting, the car required from 100 to 110 amperes, say an average of 105, at 230 volts, about 25 kilowatts, with zero volts on the hoisting motor at the start; after that, current hung in the neighborhood of 100 amperes for a little time, and gradually ran down to 40, which seemed to be about the average current for hoisting. In stopping, the current fell from 40 to zero, and reversed for a second or so. In starting down, the current went up to over 100 amperes at first, and fell gradually to about 60 where it lingered for a while and then gradually fell. On starting the elevator up, the voltage fell from 230 to 190; and on stopping, it ran up above 250; on starting the elevator down, it fell to about 180 and then ran up again to 250 on stopping. In other words, the range was from 180 to 250, or 70 volts. It is this irregularity that necessitates the separate generator for power, with another running alongside of it for lights. The power required to start the car is much greater than would be required to start the same car with rheostat control. There is no difficulty in starting such a car with a 75 per cent. increment over running power, and possibly with less. The starting and stopping of this elevator is extremely pretty; but the speed is not high, and the acceleration is very slow, and I doubt the practical value of it.

Another case is the Fahys Building in Fulton Street. In this the plant is arranged differently; there are three elevators and three separate generating sets, each comprising an engine and generator, driving a car motor, the field of the generator being varied. Here they run from, say, eight or nine o'clock in the morning to five or six o'clock in the afternoon—three engine sets turning around idly from morning till night. Some tests of this plant have been published. These show a friction card of about five H. P., *i. e.*, about 50 H. P. hours of wasted work per day. The

average total power required for hoisting was about 12 H. P., leaving 7 H. P. for the net work on the elevator. It is improbable that power is used for more than one hour daily in the aggregate: this makes 7 H. P. hours used usefully, as against 50 H. P. hours wasted,—an efficiency of 12 per cent.

Another plant on exactly the same lines is in the Sampson Building in Wall Street, where there are three separate sets, each with engine, generator and motor for three elevators.

I am sorry Mr. Leonard has not given us any results of operation. I would like very much to have him give some figures which would give some slight idea of the engineering feasibility of the thing beyond the mere illustration of the method of control.

MR. F. A. PATTISON:—I am in the same condition of mind as Dr. Hutchinson. I have always admired very much the great facility of control shown by this method, but have never been able to dissuade myself from the opinion that it is a very expensive plant to operate.

It was my good fortune to be able to compare, under very favorable circumstances, the running of the different motors with practically the same load in the Clearing House building in this city, one under Mr. Leonard's system of control—the main elevator, and the other with a load of coin said to be about the same weight, operated by the usual method. This comparison showed that the elevator operated by Mr. Leonard's system required very much more current for a round trip than the coin-lift operated under the general method.

Another plant that I was interested in was that of the *New York Herald.* But here, instead of having an engine to drive the generator, we had a motor connected with the street current. It was found that in order to cut down the expense it was advisable to have a switch inserted, so that when the elevator was not in use, this switch would be thrown and the current cut off. This was done in order to cut down the bill for simply running the machinery, that is, when it was doing no work; and to this day the elevator is operated in that way. When the elevator boy wants to use the elevator, he rings a bell and the current is thrown on in the basement, and it pays them to operate it in that way rather than to keep this machinery running while doing no work and paying the bills on the meter basis.

MR. JAMES BURKE:—I am interested in this question from a historical standpoint, and have investigated as to when this method was first used; the result may be of interest.

In 1886 a system was in use in which an Edison dynamo was used as a generator, and a 30 H. P. motor was operated therefrom, the starting and variation in speed of the motor were controlled by the strength of field of the Edison dynamo.

In 1879 a system was in use in which the speed of the motor was controlled by varying the voltage of the generator supplying this motor; the voltage of the generator in this instance was regulated by varying the speed of the engine driving it.

In 1877 a system of control was in use in which the speed of the motor was varied by operating from two generators, the voltages of which were worked in series after the motor was under way.

In 1874 a system was in use, in which 200 cells of battery were utilized for supplying current giving nearly 400 volts. The motor was started by first connecting it across a few of these cells and then gradually switching in more cells until finally the whole number was working in series.

In 1872 pumps were operated in New York by electric motors. The motors were supplied with current from batteries so arranged as to connect more or less of them in series as the conditions of operation required.

In all the instances above referred to, the speed of the motor was controlled by variations of the voltage of the supply circuit, instead of by using resistance in series with the motor. Thus the control was by voltage rather than by ohms.

Mr. Charles P. Steinmetz:—There are two features mentioned in this paper and inherent to the method, which have not yet been fully brought out. The one is the very gradual and easy start which may be made, irrespective of all questions of efficiency, which is of great advantage for passenger elevators—not so much for freight elevators. I remember a number of instances where this system was installed for first-class elevator service in order to get a gradual and easy start.

Another feature of the system is the ability to control the speed perfectly and to maintain it constant at a very low value, irrespective of variations of torque. This is a very important matter in certain instances, as for printing presses and so on, where it is necessary sometimes to run at very low but constant speed, which is not possible with rheostatic control, since by rheostatic control for very low speed, say 10 per cent. of full speed, the speed fluctuates excessively even for small variations of load.

Where a speed variation of only 25 per cent. or so is needed, it can generally be accomplished by controlling the motor field, but very wide ranges of speed cannot be covered thus.

In these two cases it is not so much a question of economy or of efficiency, nor of the first investment or the cost of operation, but the necessity of securing a method of very gradual acceleration, or a constant speed irrespective of torque at a very low as well as a high value of speed.

Mr. R. T. Lozier:—In operating large printing presses one other consideration is to be taken into account, and that is the enormous starting torque that the press requires. When it is new, it sometimes requires as much as five times as high as the running torque, and I do not think that with Mr. Leonard's system of control it is possible to provide for that starting torque. So for printing press work, it would be necessary to supply some

mechanical method for what is known as "slow motion," and which might also be employed as leverage to overcome the heavy starting torque.

Having such device it would hardly seem practical to have an independent engine and generator, and throw a load on the engine of 3 or 400 per cent. momentarily—supposing that this starting torque is necessary for that purpose—it would seem that this mechanical slow motion might be accomplished at a ratio of 20 to one. Then some form of rheostatic control in conjunction with that slow motion would seem to serve the purpose.

If it were not for the high starting torque I should say that the Leonard system for large printing presses was an ideal method; providing, of course, people are willing to pay the additional first cost, because the printing presses when once started run at full normal load, and the generating apparatus is again working at its highest efficiency.

One thing I have found in printing press work, and that is that all the presses will strike a mean load, so that taking the normal rating of the motors you will find that the load on the generators is fairly constant at perhaps 60 per cent. of the power required, if all the presses were running at once at their full capacity.

MR. WILLIAM ELMER, JR.:—It strikes me that the last speaker has confused starting torque with H. P. I do not think the H. P. required from the engine is anything like the amount which he seems to have in his mind. The starting current may be large, but if the voltage is small, the power is also small.

MR. R. T. LOZIER:—Hoe & Co. are unable to start a sextuple press, if I am not mistaken, with a 12-inch belt, but they first start it through their slow motion mechanism running with a five or six inch belt. The 12-inch belt drives the main shaft 200 revolutions; the slow motion is 10 revolutions. The long train of gears, ink rollers and the type plates setting down on the blankets form an enormous resistance to overcome in starting. The starting torque is of short duration, really a peak if you should diagram the load.

I know of one instance where the Leonard system was used on printing presses, and abandoned on account of the excessive starting loads of the press. Rheostatic control is now used in that particular instance, with a separate belting system, and operating through mechanical slow motion.

MR. LEONARD:—In reply to what has been said, I will say it is no doubt true that conditions can be obtained under which this system would be absolutely worthless. These conditions exist in such a plant as that of the *New York Herald* which has been cited. They apparently also exist in the case of the New York Clearing House, also referred to. To take an elevator that is to be operated once or twice a day, and that has no desirability of high speed nor control, and with very short lift, such as the *Herald* which is extremely short, and to attempt to apply a

method whose chief virtue, if it has one, is the susceptibility of perfect control at high speed, is of course, creating conditions which are the very best possible to make the method worthless. I do not know anything about the Clearing House elevator, for I never saw it. For instance, if the motor generator is installed for the purpose of running an elevator in a low building, such as the *Herald* building, it will almost invariably be the case, unless the system has such advantages as regards the perfection of operation and control and freedom from accidents and repairs, as to make it of comparatively slight importance whether ten or fifteen dollars more per month is paid for coal, that no such motor generator should be installed for running an elevator.

In a large office building it is always more economical to produce your own electricity for lighting by an isolated plant, and when a plant is installed for that purpose it should be used for the elevators, but not with separate engine for each particular generator, with cut-off varying from zero to full load of the engine. That is such bad engineering that you cannot expect to gain results from any system under such conditions. But if an office building be equipped with this method of control for the elevators, with perhaps two engines so arranged as to have directly coupled between them, on a shaft joining them, the necessary generators to take care of the lighting and elevators; for example, one generator for lighting and perhaps three for the elevators, which would be a common plant; four generator armatures mounted on one shaft with an engine on each end and with a coupling between the engine and shaft at each end and perhaps a coupling in the centre of the shaft; the lighting generator supplies the necessary current for exciting the fields of the elevator generators and for the field of the motors driving the elevators, and you are ordinaily producing your energy then in one cylinder, and your other engine can be made available to work in conjunction with the first, or you can split your plant in the middle and have two generators on each engine. Under conditions of that character this system would show most valuable results. The best indications of what results may be obtained if the load upon the engine be reasonably constant, is indicated by the fact that in the case of the elevators that have been cited in the Fahys Building, in which there is an individual engine for each individual generator, the maximum power in starting is about 12 H. P., and the running H. P. at full speed is in the neighborhood of about 15 H. P.; the coal bill for a month for running three elevators and for all the lighting in the building is 40 tons of pea coal per month. That figure I think will compare favorably with the lighting and elevator service in any other building in the city, and yet the conditions of the use of the method of control for the elevators are extremely unfavorable.

The figures which Dr. Hutchinson cited I cannot answer, because they appear to me to be due to such unusual conditions, that there is some explanation for, which can only be known to

himself. The idea of a generator being run by this system from 180 volts up to 250 volts is simply due to conditions that are in some way ridiculous and are no part at all of this method or system, and have nothing at all to do with it. I do not know the plant that he speaks of, and I do not know the causes leading to the results which he has stated, if they be as he has stated them. But, of course, this thing has been discussed before, and we know his views in regard to this system and he knows mine, and not much can be gained by discussing the matter between us any farther. But in the plant that was tested in the Fahys Building the figure of that test as to the kilowatt hours per car mile run was the best figure that has ever been obtained yet, so far as I know, and yet the conditions were unfavorable.

As to the historical figures which have been given by Mr. Burke, most of them of course, are well known facts that are entirely independent of, and have no relevancy whatever in regard to this method so far as I know. This method does not claim to be the only thing that was ever done in the way of varying volts, but it is limited to peculiar methods and peculiar combinations, all of which are radically different from those which he describes.

Mr. Lozier has evidently a very confused idea in regard to the question of torque and power. If there is any one thing this method will do it is to create an enormous torque with a very little power. That is its chief claim; and this armature here can be run so that it will hardly turn over and yet you cannot hold it with your hands, and it will be taking a very few watts to do it. The printing presses he describes are not as bad as he thinks, and when this method was first installed in 1891 it was upon presses which required 30 H. P. to run each of them for printing calico; and yet when all other methods by a rheostat control and other methods of that nature had utterly failed because of the enormous power required, I installed this, and started the presses invariably on a voltage of about 20 volts on an armature of a machine which was designed for 125 volts and with a current which was probably four or five times the current of full torque, because the men that were running the presses were not favorably disposed to the system. They have seen them monkeyed with for about a year with electric motors, and everything had failed, and when I succeeded in starting the press they not only put on the press the maximum torque which could be obtained under normal conditions, but they screwed the rolls down to such a degree trying to stop it, that the owner of the plant who came in at the time almost discharged the foreman on the ground that he had sprung the frames of his machine.

As to the size of the belt, that does not enable you to determine the power. The belt is merely the measure of the torque. My belt in that instance was about 12 inches, and I had to get it as tight as a fiddle-string, because my motor would start up every time and the belt would slip, and yet I was using only about four

H. P. Of course, a belt running something like 4,000 feet per minute can transmit a great deal of power, but when running slow will do very little as regards power. All that the size of the belt will give us is torque. The power is proportional to the speed of its movement.

As to the printing press which is said to have been operated by my method in this city and abandoned, I have no knowledge. I do not know where it is or what it is, and I have not heard anything of the sort myself. But all I can say is, that if it was abandoned solely because it could not get enough torque to start the press, there must have been somebody handling it that did not understand the method, because there is no doubt about starting anything that the armature will start under. The torque is practically unlimited because you have an armature starting from dead rest with a current which can be increased to a current represented by the ohms of the armature divided into the entire volts of the line, and nothing in practice ever requires such a torque as that.

As regards the operating cost of the method and the first cost of the apparatus, I do not for a moment grant that under fair conditions this is not the most economical plant to run, but I do want to point out that there are a great many cases in which the first cost, and the cost of maintenance are not the considerations which determine the choice of the apparatus. For instance, this method has been used for the handling of the turrets on the men-of-war. If anybody thinks that turrets can be satisfactorily handled by any conceivable system of rheostat control so as to enable the guns to hit anything when you get ready to shoot, all he has to do is to try something of that kind or watch others try it to learn how absolutely impossible it is to have perfect control by rheostats when you have a motor which is hardly moving. When you have to start up under the inertia and sticking of the parts, the torque will be quite high and a large portion of the rheostat will have to be cut out to get enough current to start it. The minute it is started, the torque will be about one-quarter of what it was and then the motor will run like a scared cat. In this particular case rheostat control was not in question; the question was simply of control by steam as against control by electricity. My method of control was used as representing the possibilities of electrical control, and the steam machinery was the result of a large number of the best engineers of the country working out the most refined methods for handling things of that kind. It is of very little use to have a million dollar gun with its platform, if you cannot hit anything when you shoot. The control is of prime importance, and the question whether the method costs $100 or $500 more to install is of no consequence, and the question whether it costs a few more pounds of coal to run it (which we will assume to be the fact, but which I do not grant at all, understand me) does not make any difference

in the result if they can shoot and hit something every time they shoot because of the perfect control, or if they can shoot more frequently. This method was tested, and it was found that they could make 23 distinct starts and stops of the gun under perfect control within a period of 20 seconds, and not move the gun one degree. The steam men after spending a long time getting every possible refinement of their method, finally were able to accomplish such a thing as that, by backing and filling over the line for five minutes, while by that time the object they were going to shoot at would be beyond the horizon. That is one case where the question of first cost and economy of operation are of no importance as compared with the question of control.

Another case that I have already spoken of is the question of operating printing presses of large size. The persons who have had these printing presses running with this method since 1891 would have nothing else for their presses, although every other method that was conceivable to the General Electric Company was worked upon and developed to the highest degree, but this method was installed because nothing else would do the work satisfactorily. Similarly in case of another plant that is at this time going into operation, which is to have the same method of control for the same purpose. The system is also used abroad for the handling of cloth printing presses, and I think I am safe in saying that no other method is so used. Printing presses of the ordinary kind, where the question of control is less important than in the case of printing cloth, are being run by rheostats, but in a very unsatisfactory manner. I am in the rheostat business myself and will be delighted to build them for anybody and everybody who likes them. A big rheostat is bad enough where the movement of the lever is always in one direction cutting out resistance, but for regulating the speed of a motor by a rheostat where the resistance is to be inserted, there is so much sparking and such a perfect lack of control that it only means that the person using it is not familiar with the possibilities to be accomplished by such a method as this. Of course, if you have a shunt-wound motor and then have some kind of a mechanical reduction of power between that motor and the press, that is all right, and it makes a first class system which will give most satisfactory results; but the difficulty is, that that mechanical gearing is the thing that all mechanics have been hunting for for fifty years and have not found yet, and it looks as though, for big powers, it is about as far off as when they started.

Dr. Hutchinson has asked whether I cannot give some figures with regard to the results of operation of this method of control. You will understand that this is the first time this new system has been described by me, and it has not been much used. Two or three companies have made use of it, but I have no figures at hand bearing upon the question of economy as yet, but since the system so far as the public are concerned is absolutely new, it is

not to be wondered at that the figures are not thus far available. But I will point out this; that when this motor under this system is running at full speed only one-half of the energy it uses is converted, and the other half is supplied directly from the line; when at half-speed, no energy is converted; when running at lowest speed, it is true that nearly all the energy in the motor armature is converted—perhaps one-tenth or one-twentieth of the current comes directly from the line, and the balance comes from S acting as a generator, S being driven by this other machine R acting as a motor. But I think it will not be doubted that the loss in the transformation of that energy cannot compare with the loss in the case of a rheostat, which is about nine-tenths of your total energy.

DR. HUTCHINSON:—Mr. Leonard abandons all the elevators operated by his system which he has formerly described here, including the Fahys Building; the New York Clearing House he has never seen; it is of course abominable engineering.

Mr. Leonard in his remarks, though he does not say so, seems to imply some peculiar virtue in the increase of the torque of the driving motor. You can get just as great torque by starting directly from the line as you can by that system. In one case you overload the line generator, and in the other case you overload the machine S. It is the same degree of overloading, and the only question is of line drop. There is no peculiar virtue by which the driving torque can be multiplied by this arrangement. As I understand his position at present, he does not make any special claim for economy in the system. Practically he admits that economy has nothing to do with the question, but that where ease of manipulation is essential or to be desired, that this system should have the preference over any other. In that I agree with him thoroughly.

A comparison of the energy expended in moving a ton a mile on an elevator and on a tramway may be interesting; for an elevator of about 25 square feet platform area, running about 250 feet to the minute, it requires about 2000 watt-hours per ton mile; an ordinary tramcar requires about 300 watt-hours per ton mile. This is due, of course, to the average 50 per cent. grade of the elevator car.

MR. LEONARD:—I am not surprised that Dr. Hutchinson has retreated from the position which he assumed a few months ago when he made comparisons before us on the basis of kilowatt hours per car mile. The reason is because of the favorable showing of the elevators in the Fahys Building, where tests showed a power required of only about 60 or 70 per cent. of the most favorable figures he claimed. That, of course, makes it necessary for him to change the basis of figuring. He knows too well. and I hope none of you will be misled by his statement as to torque. I made no claim of securing a greater torque than can be obtained directly from the line. What I claim is, that

this system secures a very large torque from a constant potential system with very small amount of power, which cannot be done by any rheostat system. Of course, you can get just as much torque from a constant potential system with rheostat control. It is purely a question of current through the motor armature; but I will produce that current for starting the motor with one-twentieth part of the energy required in the case of rheostat.

He said the only consideration was the drop in the line. The drop in the line, while important, is of comparatively slight importance; the power used and the control are the main factors.

Another point which has not been mentioned particularly is the question of restoration of energy to the line. Now, I cannot give any figures for this, except as regards elevators, and as to elevators, the system has been used but comparatively little and then not under the best conditions; but I want to remind you that one of the oldest electrical concerns in the world, and one proven to have the best judgment—the Siemens and Halske Company—are putting in plants abroad, employing shunt-wound motors, for the reason that such a motor has the power to restore some energy to the line. This is a very important matter, and, in the case of this system, can be brought to the very highest degree of perfection. Of course, in the operation of this motor if you are on long runs, without stopping or starting, the transformer would be entirely cut out of circuit and not in service at all, if you did not need the boosted volts for the full speed.

Another point I have not mentioned, but which will throw some light on the question of first cost, is this:

The variation in speed of motors to-day is accomplished to a certain degree by the variation in the field, and all that can be done in that way can be accomplished as an additional feature for this method. By weakening the field of S. this machine M will go faster with regular normal fields, and then by weakening the field of M we can go still faster. So that, if we have to run at high speed on a level, where the torque is comparatively small, we can make use of an apparatus which will be even smaller than I have hinted at in the paper, by reason of the fact that the current can be held within sparking limits even in the weaker fields and yet give enough torque for the high speed service on the level.

MR. CARICHOFF:—I wish to cite another example where the current produced by shunt-wound motors, may be restored to the system. In the Siegel-Cooper building there are 16 passenger elevators, capable of carrying from 40 to 50 people each. They are geared to carry 5,500 pounds at 150 feet per minute. The current required to carry 5,000 pounds net on the hoisting ropes of one of these elevators, if I remember rightly, is 110 amperes; the same machine on the down trip will restore 60 amperes, and as all of the elevators are connected electrically, one machine ascending is assisted by some other machine descending.

Half the machines going up and half coming down at the same

time, require 25 amperes average per machine, each with 5,000 pounds net on ropes. In fact this whole plant consisting of 16 passenger elevators and five sidewalk elevators, runs with less than 400 amperes of current at 230 volts.

The elevators run about 150 feet a minute, and calculated on above basis the energy required is 3.4 kilowatt hours per car mile of travel; if these elevators were carrying 2,500 pound net, and running 300 feet, they would require only 1.7 kilowatt hours per car mile.

Similarly the same number of foot pounds, viz: $150 \times 5,000$ or 75,000, may be resolved on a properly geared machine into 1875 pounds at 400 feet per minute, and the energy per car mile becomes $3.4 \times \frac{150}{400}$ or 1.27 kilowatts per car mile of travel. Again, if the net load is one half of 1875, or about 900 pounds on the ropes, as was given in the reported test of the elevators in the Fahys building, the energy per car mile of travel becomes one-half of 1.27 or 0.635 kilowatts per car mile of travel.

The above inferences are based on continuous running with constant load. Starting and stopping, and changing load, will of course introduce modifications.

Allowing 50 per cent. increase for frequent starting, we still have less than one kilowatt per car mile of travel for a group of rheostat controlled elevators, or for one or more with storage battery, operating at same load and speed as those in the Fahys building, against 2.7 kilowatt hours per car mile as reported in a test of that plant, for a *combination that does not waste energy in resistance.* It must be borne in mind that the load here considered is the net pounds on the hoisting ropes; the weight carried in the car may be more or less than this net load according as the system is over-counterweighted or under-counterweighted.

Mr. Leonard:—The test of the elevators in the Fahys Building showed 2.7 kilowatts per car mile; but, of course, what figure we might get is one of those things we can all guess at. I think that the last speaker said 1.7, well, I guess that with my system and with the 16 elevators all driven by one common source of power the figure would be cut down to 1.2.

Dr. Hutchinson:—Mr. Leonard ignores the fact, in claiming that he gets his torque with less power at the main machines, that in the case I cited, the New York Clearing House, this is not true. He brushes aside my figures, by saying something was wrong,—in which I agree with him; it is his system. I repeat, at this plant the starting current is over 100 amperes, at 230 volts, for a car having about 25 square feet floor area, running empty and accelerating slowly.

Mr. R. T. Lozier:—Mr. Leonard is of course quite correct about the starting torque not representing the H. P. obtained by multiplying the amperes taken in starting, and full voltage at terminals when motor is running at full speed. I have observed with him

both the amperes and volts on one of these large presses at starting and when running at fnll speed.

I think that every manufacturer of dynamos and motors is in favor of the Leonard system, because by its installation we sell more dynamos and motors—provided we can get them in. Let me state that in the instance cited where his system was used and abandoned, I did not make the trial.

MR. LEONARD:—About that system that was installed and given up; that rankles a little bit; I don't know where it is or what it is, but if it is the printing press which I rather fancy it to be—and I think I have some confidential information which enables me to locate it—it is a case in which there was used a generator built some—20 years ago I was going to say, but at any rate a great many years ago—of a type that is obsolete, and not one that was well adapted to large fluctuating currents. It has a very small commutator, the type of commutator in vogue in 1885. It was not a generator such as would be suitable for running any kind of elevator or printing press, if it is the case I think of: and, it may be, the fact that the old generator was used for the purpose, rather than a generator of modern design, will account for its not being satisfactory and therefore abandoned.

[Adjourned.]

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, December 16th, 1896.

The 111th meeting of the INSTITUTE was held this date at 12, West 31st street, and was called to order by President Duncan at 8 P. M.

The Secretary announced the election of the following associate members by the Executive Committee at the meeting in the afternoon.

Name.	Address.	Endorsed by.
ADAE, CHAS. FLAMEN.	X-Ray Laboratory, P. O. Box, 2809; residence, 36 West 35th Street, New York City.	A. L. Riker. T. L. Proctor. W. L. Bliss.
BLUNT, WILLIAM W.	Electrical Engineer, Westinghouse Electric Co., Ltd., 32 Victoria St., St., London, Eng.	Chas. F. Scott. A. J. Wurts. C. A. Bragg.
BYRNS, ROBERT A.	98 Ferry Street, Lafayette, Ind.	H. B. Smith. E. F. Norton. W. E. Goldsborough.
CRAIN, JOHN JAY.	Electrician's Helper, Niagara Falls Power Co., Niagara Falls, N. Y.	Edw. L. Nichols. Harris J. Ryan. C. P. Matthews.
HUMPHREY, HENRY H.	Consulting Electrical Engineer, Bryan & Humphrey, Turner Building, St. Louis, Mo.	F. G. Schlosser. J. E. Randall. W. F. White.
KITTLER, DR. ERASMUS.	Elektrotechnisches Institute, Darmstadt, Germany.	Carl Hering. Ralph W. Pope. A. A. Knudson.
LATHAM, HARRY MILTON.	Member of Engineering Staff, Crocker-Wheeler Electric Co., Ampere, N. J.	S S. Wheeler. Gano S. Dunn. F. M. Pedersen.
STEWART, ROBERT STUART.	Supt. of Lines, Public Lighting Commission, 440 Jefferson Ave., Detroit, Michigan.	Alex Dow. C. F. Brackett. Jesse M. Smith.
SUTTON, FRANK.	Consulting Engineer, 27 Thames Street, New York City.	M. I. Pupin. F. B. Crocker. Max Osterberg.

Total 9.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners, November 11th, 1896

LOOMIS, O. P.	Electrical Engineer, Bound Brook, N. J.
SCHOEN, A. M.	Electrician, So. Eastern Tariff Association, Atlanta, Ga.
FIELD, H. G.	Consulting Electrical Engineer, Detroit, Mich.
MCMEEN, S. G.	Engineer, Central Union Telephone Co., Chicago, Ill.
MCCROSKY, J. W.	Electrical Engineer, La Capital Tramway Co., Buenos Aires.
FORTENBAUGH, S. B.	Ass't Prof. of Electrical Engineering, University of Wisconsin, Madison, Wis.
PARKER, LEE HAMILTON.	Ass't Engineer, Railway Dept. General Electric Co., Schenectady, N. Y.
BRINCKERHOFF, H. M.	Electrical Engineer, Metropolitan West Side Elevated R. R., Chicago, Ill.

Total 8.

THE PRESIDENT:—The subject for discussion this evening, gentlemen, is "The Röntgen Ray, and its Relation to Physics," and the discussion is to be opened by Professor Rowland.

A Topical Discussion at the 111th Meeting of the American Institute of Electrical Engineers, New York, December 16th, 1896. President Duncan in the Chair.

THE RÖNTGEN RAY, AND ITS RELATION TO PHYSICS.

(A Topical Discussion)

OPENING REMARKS BY PROF. HENRY A. ROWLAND.

Mr. President and gentlemen: A gentleman asked me a few moments ago if I knew anything about the X-rays. I told him no; that what I was going to tell to-night was what I did not know about the X-rays. I do not suppose anybody can do much more than that, because all of us know so very little about them. We were very much surprised, something like a year ago, by this very great discovery. But I cannot say that we know very much more about it now than we did then. The whole world seems to have been working on it for all this time without having discovered a great deal with respect to it.

I suppose it is not necessary for me to go into the history of the subject. We all know it; how Lenard first, probably, discovered these rays, or discovered something very similar to them; how Röntgen afterwards found their particular use, their penetrating power, and so on, although Lenard had found something similar to that before. It is thus not necessary for me to go into the history of the matter, but simply to go over, to some extent, what we know with regard to these rays at the present time. First, there was a discussion, some time ago, as to the source of these rays. Röntgen thought that their source was any point that the cathode rays struck upon; and you will remember that when we first knew about these rays they were often called cathode rays. Many persons thought that the cathode rays came through the glass, and Lenard's first idea was that they did come through his little window, and it is probable that they do at the present time. But the kind of rays that we are considering are very different from the cathode rays. As to their source, I believe it was finally determined that they came from points where the cathode rays strike. At the same time I was rather opposed to that. In one of my tubes I found that the rays came from the anode. I had

only the ordinary assortment of Crookes tubes, and one of the tubes had aluminium wires which were a millimetre apart. In this one the source of the rays was a point upon the anode—not upon the cathode at all. It was a very small point. The photographs which I obtained by that tube are sharper than any I have ever seen. They are so very sharp that in estimating the shadow of an object, I determined that the point could not have been a thousandth of an inch in diameter. Therefore the source in this case was a very minute point upon the anode, and that point was near the cathode. I suppose some of the cathode rays might have struck upon it, and it might have obeyed the law that the point where these X-rays are formed is the point on the anode where the cathode rays strike.

I had another very interesting tube, and I was going to bring some of the photographs here to-night; but I thought they were so small that it would be almost impossible to see them. I tried three cases in this tube: First, the case where the cathode rays strike upon the anode. In that case I got very many Röntgen rays. Then I tried the case where the cathode rays strike upon an object—a piece of platinum. I did not get any rays whatever then. Now, some people say that they come from the point where the cathode ray strikes. I did not get any whatever. In this case the cathode rays struck upon a piece of platinum in the centre of a bulb, and no rays were given out by the anode either. Therefore I seemed to have a crucial experiment in each; I seemed to have the case where the cathode rays strike upon the anode, and I got plenty of rays. Then I had the case where the cathode rays strike on a piece of platinum, and I did not get anything at all. Then where the anode itself was free and no cathode rays struck it, I did not get anything from it. It seemed to me as if the source was most abundant when the cathode rays struck upon the anode; and that is the theory, we know, upon which nearly all tubes are formed at the present time. You have the focus tubes in which you focus the cathode rays upon the anode, and in that case you have a very abundant source of rays; but I do not believe you ever could get as small a source of rays as I got with that first tube, where I had a source of a thousandth of an inch diameter. Having such a small source of rays, it gave me a limit to the wave length, if there were waves at all. As to whether there are any rays produced where the cathode rays strike on any other objects, we know that there are very feeble ones. It seems to be almost necessary in order to get an abundant source that you should have cathode rays strike on the anode. However, that is a point of discussion. Now, as to the source of electricity, we have generally the Ruhmkorff coil. There is one source of which I saw a little note in *Nature*, where a man had used a large Holtz machine with very good effects. Now it is

very much easier for many persons to use a Holtz machine than to use a Ruhmkorff coil. There are many cases where one cannot have a large battery; and this man said that with the Holtz machine he got as great an effect as with the Ruhmkoff coil. Then we have the Tesla coil, etc. By the way, speaking of the Tesla coil, I am not sure but that you might look back and find that it is very similar to the Henry coil. Henry originally experimented on the induction of electricity, transmitting a spark of electricity from one coil and getting a spark from another, and the Tesla coil is something like that, except that it is made so as to produce a much more voluminous spark.

We all know the properties of the Röntgen rays—they go in a straight line. Every effort to deviate them from a straight line, by any means whatever, has failed, except that when they strike upon an object they are reflected. Now, it is a question for discussion as to whether there is any regular reflection. They strike upon an object, and you get something from that object which will affect a photographic plate. Are those rays which we get from the object Röntgen rays still, or do the Röntgen rays strike upon this object and generate in it some sort of rays which come out, different from the Röntgen rays, and affect the plate? We do not know that. Neither are we quite positive whether there is any reflection of the rays. We know there is turbid reflection—you may call it—rays strike on the object, and the object becomes a source of rays of some kind. Nobody has ever found out what sort of rays come from the object. Something comes from it, and we generally imagine, and indeed we often state, that they are Röntgen rays that come off the object. But we have good reason to suppose that they may be something else; and there may or may not be regular reflections; some persons say there are and some that there are not. I have seen some photographs made in this city which indicated regular reflections. At the same time I would not be positive as to this action. It is rather doubtful. It is a point to be determined.

Then the fluorescence—that is the way Röntgen originally found the ray. You know the way they produce fluorescence—the photographic effect—you all know that. You all know that the magnet does not affect them—does not turn these rays from a straight line.

The polarization of the rays: We have no evidence whatever as to the polarization. If they were very small waves, transverse waves, like light, we ought to be able to polarize them. Becquerel, by exposing certain phosphorescent substances to the sun, obtained from them certain rays which penetrated objects like aluminium, etc. But these rays were evidently small rays of light, because he could polarize them, and he could refract them. But we never have been able to discover that there was any such effect in a Röntgen ray. Some persons

have claimed that they got polarization; but if there ever was any polarization, it is very small, indeed.

One of the principal advances in respect to these rays is that made by J. J. Thomson, in considering the electric discharge of bodies. He has published most valuable results with regard to this effect. When the rays fall upon a gas, they affect the gas in some way so that it becomes a conductor. Now, you can subject the gas to these rays and allow the gas to go through a tube off into another vessel, so that it will discharge an electrified body in that vessel. But he has found the most interesting result that it will not continue long to affect these bodies. After one has allowed a certain amount of electricity to pass through it, it then becomes an insulator again. That is easily explained by the Röntgen rays liberating the ions, and only a certain amount of them. Just as soon as these are used up in the conduction, then the gas ceases to conduct. So that a certain amount of gas will conduct a certain amount of electricity, and then it stops conducting. That is a most interesting result. It is one of the great advances we have made since Röntgen's discovery. Röntgen knew nearly all we know now about these rays. We have discovered very little indeed; but that point I think we have at least discovered.

Then it is said that these rays affect a selenite cell in the same way that light affects it—it changes the resistance of the selenite cell.

Of course, we are only considering the theory to-night; at least I am, and we do not have to consider the bones, and so on. I have had some students at work in my laboratory, and it was with the utmost difficulty that I kept them from photographing bones. Bones seemed to be the principal object to be photographed by the Röntgen rays when they were first discovered, and I suppose it is the same now. Most people connect Röntgen rays with them; but I do not intend to say very much about them.

Now, one important point with respect to these rays is as to whether they are homogeneous. Are they like light which can be divided up into a large number of different wave lengths, or are they homogeneous? There seems to be a great deal of evidence that they are not all the same; that one ought to get a spectrum of them in some way. We can filter them a little bit through objects. After they are filtered, they are probably a little different from what they were before, and some objects probably let through different rays from others. In *Nature* Mr. Porter, I believe, has shown experiments upon that. He divides rays into three kinds. At least he finds that under certain circumstances the rays will penetrate bones better than in other cases—bones or any other object. They have more penetrating power, and they go through many of those objects that ordinarily stop them. By heating the tube, and by various

arrangements of his spark-gaps, etc., and putting little wires around his tubes, and so on, he can cause them to generate different kinds of rays. That is a very important point, if it is substantiated, and there seems to be little reason to doubt that a number of rays really do exist; that whatever they are that come from the object, they are not all the same; some of them penetrate bodies better than others, and very likely some one will get up some sort of filter that will filter them out, and allow us to use them and to find if they have different properties. At the present we are rather in the dark with regard to this point.

Now I come to the theory of these rays. What is the cause of all these phenomena? There was a time when we were rather self-satisfied, I think, with regard to theories of light. We thought that Fresnel and others had discovered what light was—some sort of vibration in the ether; we called it ether; if it had these waves going through it, then it would produce light, and we were pretty well convinced that the waves were transverse, because we would polarize them; so that we began to be satisfied that we knew something about light. Then Maxwell was born, and he proved that these rays were electro-magnetic—very nearly proved it. Then Hertz came along and actually showed us how to experiment with these Maxwell waves, most of which were longer than those of light. At the same time they were of the same nature. Well, we got a rather complicated sort of ether by that time. The ether had to do lots of things. One must put upon the ether all the communication between bodies. For instance, what communication is there between this earth and the sun? Why, you have light coming from it and heat. Radiation you might call it all. Then some people thought they discovered electro-magnetic disturbance from the sun. Sometimes they have seen a sun spot and noted a deflection of the magnetic needle on the earth. Very likely that is true. I don't know that they have discovered any electrostatic effect. But we know that electrostatic effects will be carried on through as perfect a vacuum as you can get. Then we have gravitation action too. Now, we have got all these things—electro-magnetic action, light which would be an electro-magnetic phenomenon, and then we have gravitation, and we have got to load the ether with all these things. Then we have got to put matter in the ether and have got to get some connection between the matter and the ether. By that time one's mind is in a whirl, and we give it up.

Now we have got something worse yet—we have got Röntgen rays on top of all that. Here is something that goes through the ether, and it not only goes through the ether but shoots in a straight line right through a body. Now, what sort of earthly thing can that be? A body will stop light or do something to it as it goes through; but what on earth can it be that goes through matter in a straight line? Why, our imagina-

tion doesn't give us any chance to do anything with that problem. It is a most wonderful phenomenon. We can indeed suppose that they are ultra-violet light. Indeed, we can get a limit to the wave length to some extent. Nobody, however, has ever proved that the Röntgen rays are waves. But we can get a limit of the wave length if they are waves, because when I have a tube that gives me a shadow which is only a thousandth of an inch broad, or rather from the greatest intensity out to clear glass a thousandth of an inch broad, I can calculate the wave length of the disturbance that would produce such a shadow. It has got to be very small indeed; one knows that right away, because any ordinary light would make a few waves at the edge of the shadow, and by measuring those waves you could get the wave lengths of the light waves. But there was no appearance whatever on any of my photographs of any such phenomenon as that. I did not have any of these waves at the edge of the shadow whatever. It went directly from blackness to light. But putting it under the microscope and measuring from almost imaginary points, from lightness to darkness, I could get a limit to the wave length. Now, as to that limit, I published it in one of the journals six months ago, or more, and it came at about one-seventh, I think, that of yellow light. Others have determined the wave length and got even below one-seventh that of yellow light. Some have got one-thirtieth that of yellow light, and so on. Some of them I am rather doubtful about, because they say they have bands. If they have bands and defraction bands, that would prove instantly that the Röntgen rays are waves. But I have never seen the slightest phenomenon of that sort. It is very doubtful that it exists, and those persons who have had it will have to show their photographs very clearly to make us believe it. And therefore we have no evidence whatever that the rays are waves. At the same time we have no evidence that they are not waves. They might be very short waves—infinitely short waves. Let us see what would happen if they were infinitely short waves. They might be so very short as to be too fine-grained for any of our methods of polarization or reflection. Waves are reflected from a solid body—regularly reflected, because they interfere after they come from the body. You can get the direction—the angle of incidence equals the angle of reflection; you can get that by means of considering them as waves and as interfering after they come from the object. Well, if the object, however, is a very rough sort of thing compared with the wave length, you will not get a regular reflection. That is what might happen in the case of Röntgen rays. And then again, with regard to refraction of the light, the theory of refraction which comes from considering molecules imbedded in the ether will give you some limit. When we go beyond that limit, we get no refraction. The bending of the violet rays increases up to a certain point

and then goes back. We have a case of anomalous refraction very often in some substances like fuchsine, aniline dyes, and so on. Therefore the action of refraction can be accounted for by having very short waves. But when we treat of the theory of the case we have the little molecules of a gas knocking against each other, and they can only go a little distance. We call that the free path of the gas—a very small distance in the ordinary air. Those molecules cannot go more than this very small distance before they stop. Well, now, why should little, short waves of light pass through the gas and not be stopped too? When the waves are very short indeed, it seems to me that the object would be entirely opaque to them, because they would strike upon those molecules, unless they could pass directly through the molecules. You would therefore necessarily have these little short waves going directly through the molecules, which we generally think is almost impossible in case of light. And that is one very great objection that I have to that theory.

Then we have another theory—that these are not transverse waves at all; that they are waves like sound, and very short indeed. Well, what would happen then? If they are very short indeed, you have the same objection: They would all strike against the molecules, and they would be dispersed very quickly. The shorter the wave lengths, the more they are dispersed. Take, for instance, short waves that bob against a boat and are reflected back. Then, if you have a big, long ocean wave, it sweeps around a boat and goes on without being troubled by the boat at all. The shorter the waves, the more they are bothered by the boat, and so it is with respect to other waves—the short waves would probably be stopped by the molecules. So I do not see what we can do with regard to it in that respect. According to Maxwell's law, waves like sound do not exist in the kind of ether that he suggested. But that is all based upon a certain theory that the lines of force are always closed. He introduced into his equation an expression which indicated that every line of force was a closed path coming back upon itself or ending in electricity, one or the other. If we throw out that equation, then we can get this kind of compressional waves in the ether. Now, it is not at all impossible that they exist, and as to whether they would go through molecules any better than light waves do, nobody can tell; but it is possible that they might. But if there are waves at all, they must be very short waves. You cannot get over that fact.

Then, of course, you have the other theory—of little particles of matter flying out from the body, passing through the glass and all other bodies, until they reach a photographic plate or any other place where we are notified of their presence, and these little particles make their way through the air or any other substance. Now, why should not the little particles be stopped

very quickly by bodies as well as if the rays were waves? You see we are in trouble here too. Why are not the waves stopped? Why are not the little particles stopped? Stokes has given some sort of a theory with regard to this—that, instead of having a wave motion in the ether, the rays are impulses—a sudden impulse—one wave, for instance—not a series of waves at all, but one impulse coming out from the tube. I think if he had seen any very sharp shadows obtained from the Röntgen rays he would not have given that theory. He probably has seen only those very hazy outlines that very many persons take for Röntgen photographs. But if he had seen any very defined ones—very sharp ones—he probably would not have given that theory, because if the Röntgen rays are waves at all, they must be short, and there must be a long series of them to make sharp shadows. This is why Newton gave up the wave theory of light. You remember he gave up this theory because he found that light went straight past an object instead of curving around into the shadow as much as sound does. But he was not quite up to his usual pitch when he made that statement, because if he had thought a moment he would have seen that very short waves will go more nearly in a straight line than long ones. But any single impulse, such as Stokes suggests, would go into the shadow. The only wave motion that would go in a straight line is a series of waves, one after another. Therefore, these rays cannot be single impulses coming irregularly.

Prof. Michaelson has suggested a theory of rays based on something like vortex rings in the ether. Now, if we have an ether that can carry on light waves and electro-magnetic waves, it cannot be a perfect fluid; it has got to be something else. You cannot very well imagine vortex rings in such an ether. So that we are met at every point by some objection. We have been studying light for hundreds of years; we are not anywhere near satisfied with the theory yet, and we cannot very well be expected to be satisfied with the theory of Röntgen rays in one year.

Well, I think that is all I can say with regard to the subject, and I hope the other gentlemen who are to carry on the discussion will satisfy you on all these points that I have brought up and left unanswered.

Prof. Elihu Thomson:—Mr. President and gentlemen of the Institute:—I have been very much interested in the expression of opinion by Prof. Rowland as to the nature of these rays. I can certainly second his statement that we know very little about them; that is, as to their real nature, and the more facts we accumulate they seem to be carrying us, if anything, farther away. We may have a dozen different theories, and we do not yet seem to have any proof of any of them. I have not, however, given very much time to considering that side of the question; I have been working somewhat, as leisure time allowed, in finding the

conditions under which these rays were produced, and in obtaining them, if possible, in great amount.

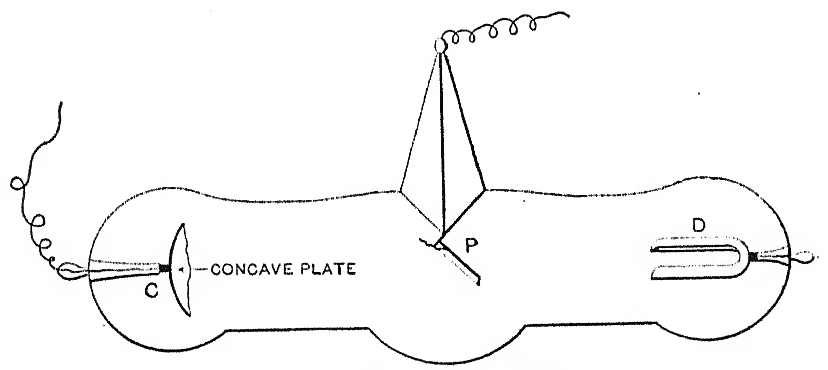
I can hardly agree with what Prof. Rowland says in regard to the rays not being produced when the cathode rays strike anything but an anode, unless this qualification be admitted—that for the time being, the thing struck becomes an anode by induction. That, of course, may be the explanation; that may be the way out; because we have many tubes which have mounted in the interior an insulated piece of metal—I had a very active tube in which an insulated piece of platinum was mounted opposite two concave pieces of aluminium which were made alternately the cathodes. This insulated piece mounted on a glass stem, without any connection on the outside, was a vigorous source of rays, and unless it became an anode by induction, we must admit that a piece of metal bombarded becomes a source. So the glass of the tube may become a source by being bombarded, and a source in all directions. The rays are transmitted through the glass; they are transmitted back from that point also laterally, and in fact in all directions. I have come to regard it as a settled fact, that if the cathode rays strike a piece of any substance, and particularly a dense substance like platinum, uranium or iridium, and these cathode rays are directed in a right line toward the surface, they produce Röntgen rays. If, however, they are diffused, as by crossing the focus of the cathode and then spreading out, as when the vacuum is a little too low to allow them to go on as a jet, then the rays cease to be produced. That condition is present in a tube too low in vacuum, and you can generally recover it by working the vacuum up.

As to the methods of excitation of tubes, I have found that static machines, induction coils, high frequency coils and various other apparatus for giving high potentials, all under proper conditions, are about equally satisfactory. For a static machine to be used, it must be of large capacity. In other words, it must have a large watts output relatively for such machinery, and a multiple plate machine therefore, driven with high speed, as one made with hard rubber plates driven 2,000 revolutions and say 24 plates, is a wonderful source of discharges for the tubes. It will keep the platinum hot in the tube, making a bright spot on the platinum. The spark itself between the terminals of such a machine will almost instantly set fire to combustible materials put in between the terminals. A vigorous induction coil gives, of course, uni-directional discharges, like the Holtz machine, except that they are intermittent, whereas the Holtz machine can keep a Crookes tube apparently continuously lighted; that is, the intervals between the discharges are so exceedingly small that the most continuous possible effect is obtained when the static machine is used. It has, at it were, a constantly impressed electromotive force, and the output of the machine is, as it were, a constant current under that electromotive force, but no doubt

divided into intermittent discharges at a very high rate in passing the tube. An induction coil driven with 50 to 100 breaks per second, is an admirable source of rays when the tube is proper for it, and in that case what is called a single focus tube is used. When we come to the high frequency alternating discharges we have the double focus tubes with two cathodes generally placed opposite each other, and the rays from which bombard in common a piece of platinum in between, and if that platinum be made in the form of a wedge, with a somewhat acute angle, the two bombarded spots or sources from either side may be so near together as to give practically one focus for ordinary uses. In that case the tube, if exhausted properly, is a vigorous source of rays.

But the most effective method of excitation which we have yet been able to use has been one of which the public has heard nothing so far, and I mean to speak of it now. It was arranged by Mr. Hermann Lemp (whose name is familiar to many of you) in this way: He simply took a 12-inch inductorium, a coil giving a 12-inch spark ordinarily, and excited the primary with alternating currents at 125 cycles. This gives in the secondary, as in any step-up transformer, a great increase of potential, such that we may find that the spark darts five or six inches between terminals, and, of course, after it is started, there is a continuous arcing somewhat difficult to stop unless you blow it out or shut off the current, which latter is probably the easiest way. Now this high potential discharge of the secondary is, of course, an alternating current—an alternating current somewhat of the same wave form as the impressed primary wave. But if we rotate, by a little synchronous motor, a break piece which picks out one direction only of the secondary discharge and leaves that in the other direction open-circuited, then you see we have an admirable source of uni-directional discharges of great power. Of so great power indeed are they that you would not dare to put them upon your Crookes tube without modification. You must put in a high resistance, such as a water resistance—a long glass tube filled with water—to cut down the flow which would destroy almost any tube you might try. The commutating device made properly simply consists, for example, of two insulated terminals in series with the discharge, and a connecting wire revolving synchronously between them. With a spark gap between the wire and fixed terminals, of course we do not need friction or any contact. You are then chopping off the tops of the waves or are taking the very highest potential of each and every wave all in one direction, and you are also giving them a spark gap between the fixed terminals and revolving wire which is favorable to the generation of the rays. The spark gap regulates itself in a measure, because just as soon as the potential is such that it can easily jump a gap, then the commutator wire anticipates the discharge and the current leaps the spark gap. If there should

be a weaker discharge, the wire comes up nearer to the terminals before the gap is jumped, so that in this case the discharges are wonderfully uniform and you get them at the rate of 125 per second, which is a rapid rate, and excites tubes wonderfully well. With this means of excitation which I experimented with about a week ago for the first time, I was astonished at the results obtained. Unless you are very careful, you melt right through the platinum electrode. Half a second might be sufficient. In fact, we did that, but it did not hurt the tube any as a source of rays. There existed just back of the platinum plate a brace of thicker platinum which did not melt. The rays did not strike that thicker piece squarely; they only struck it on about one-half the area of the hole formed in the platinum sheet. A singular thing occurred in this case. I will draw a sketch of the tube which was used (see Fig. 1.) Here, at P, was an inclined plate of platinum sealed in from the side, supported from this side,



GLASS X RAY TUBE

FIG. 1.

and with a little rib of platinum on the back, and at this end, D, a dummy terminal of no particular use. It was simply put in there because it was thought it might be of some use—and that is sometimes a good thing to do in these tubes. Then we had the concave cathode, at C. Now the rectified current was sent between these C and P and a hole was instantly bored through the platinum P and the rays struck the little rib back of it. The rib was about one-half exposed to the hole after it was made, and half the size of the hole was opened for the passage of rays clear through—cathode rays, of course, in this case. The result of this was that in about three seconds this piece of aluminium, about an inch and a quarter or an inch and a half wide and a thirty-second thick, was red hot. It was repeatedly heated in three or four seconds by the cathode rays that had apparently passed this rib and gone through the hole. This shows the enormous vigor of the cathode rays. When this tube was used there was a white hot area all around the hole in the platinum plate.

In using the fluoroscope, or fluorescent screen in the dark, I found that a most intense sharp shadow of the bones in the hand, and a shadow comparing with those obtained by photography was thrown upon the screen, and the details were beautiful—although the image was formed within only a short distance of the tube. But the astonishing thing was the flood of light obtained, and the fact that you could take the screen away and for many minutes afterwards see that shadow glowing—or rather the space around it on the screen glowing. I would like to read here in this connection some few notes made at the time I quote:

"When the vacuum was so high that but very little fluorescence existed on the sides of the tube, the rays got through four thicknesses of iron over one thirty-second thick—that is, an actual thickness is three-sixteenths—and cast strong shadows of a brass disk or plate about an inch thick. When the vacuum is lowered the screen becomes very much more luminous, but the shadow almost disappears."

This means that there are apparently produced rays which are not going in a direct line; they are being diffused, and this seems to indicate that a different wave length and rays more diffusible by the matter of the iron are produced.

"The same is true, but to a less degree, with the hand or with the shadow of any object. With eight thicknesses even, the tube having a high vacuum, the screen is lighted and a fine shadow thrown on it. This makes a total of three-eighths of an inch of wrought iron, working about six inches from the bombarded platinum. A heavy cast-iron transformer cover with a brass name plate showed the name plate clearly through the iron. Two metal pieces—cast-iron—nine-sixteenths, gave, with a higher vacuum, an enormous amount of rays, but no distinct shadows could be seen passing through these plates, owing probably to the fact that the metal may be a very strong diffuser of the rays."

Now that last statement is a very curious one. Take two heavy plates of iron about nine-sixteenths thick and slide them over each other so as to get over half the screen a double thickness, and it was very difficult to discern any difference between the part with a single plate and that with the double thickness between it and the source. If the current was cut off after the tube was excited, the luminosity of the screen disappeared. There is only one other explanation of this phenomenon that I am able to offer, and I offer it, having experimented too little to say whether it is a correct explanation. It is just possible that the screen is not illuminated by the rays coming *through* the iron, but from the diffusion of the rays from the operator's body. The only way to avoid that diffusion would be to get into something like an armor plate casing and work through a hole, so as to be able to cut off all these diffusion rays that are thrown backward.

Prof. Rowland has told you about the general characteristics

of these rays in their relation to the cathode rays, and I wish merely to call attention to one or two additional points. In working a tube, say a single focus tube, with an inclined plate of platinum, used as anode, it has always been noticed that when it did its work vigorously, there was an area of fluorescence on the glass wall extending from the plane of the platinum and covering the whole of the walls on the bombarded side. This illumination is uniform apparently all over those parts of the glass walls of the tube that are centered around the bombarded spot on the platinum, and that uniform illumination never comes unless you get Röntgen rays. They are evidently produced by the Röntgen rays passing the glass outwardly and illuminating or making the glass fluoresce. But, curiously, you can see such fluorescence in daylight or in a room fully lighted, but if you pulverize some of the same glass and make a fluorescent screen of it—experimenting with different thicknesses, you do not get more than the faintest action even when you put the screen close to the glass of the tube. What does that indicate? It seems to me there is no escape from the conclusion—that there are some rays that strike the glass which do not go through—that they are the kind to which the glass is opaque, or nearly so, and very little of them get through; whereas, the Röntgen rays that do get though, having passed through glass, (being filtered as it were) can now go through glass again or other things of the same nature. It is the quantitative effect of the fluorescence that leads us to such a conclusion. We also find that cathode rays produce fluorescence wherever they strike, and this leads us to inquire whether in doing so they always produce Röntgen rays first, or these other rays which are lower than Röntgen, or whether the cathode ray does alone produce the fluorescence. I think that this question would be one of the most difficult to experiment upon or to decide. That there are varieties of Röntgen rays that differ in some way (whether in wave length or what not it is hard to say) is undoubtedly a fact beyond all question. They cannot be divided, in my opinion, into X — 1, X — 2, X — 3, or to put it differently, into flesh, wood and metal rays; but they probably represent, if they represent anything, a sort of gamut—a variation upward and downward in the scale. In working the tube Fig. 1 and lowering the vacuum somewhat, using an iron plate three-eighths thick and the screen in front, you get very little effect at first or not until the tube works up, and it works up in half a minute or a minute. You then see the screen getting brighter and brighter all the time until the screen is illuminated quite strongly. This evidently indicates the production at the last of apparently a different sort of rays which penetrate the metals even when of considerable thickness. But if you put your hand or any thin metal object back of the heavy plate when the rays pass freely, you get a very faint shadow indeed, showing that these rays which get

through the metal, get through the hand, if anything, more easily. They are the rays which will get freely through a man's body, perhaps through great depths of it. It probably requires these very rays to pass through any great thicknesses and this may account for the fact that it is very difficult to get any strong impression of the vertebral column, for example, in the body, because such of the rays as are able to pass through great thicknesses of tissue have also a high penetrating quality, even for bone.

Another fact which I noticed in experimenting with this tube (Fig. 1) is, that adding sheet after sheet of iron, each a little over $\frac{1}{32}''$ thick, beginning at 1, then 2, 3, 4, when I got to about four sheets my object shadow was black and very clear and distinct. When I got to five or six sheets, that shadow was beginning to be faint and blurred, and at eight sheets could just about be distinguished with care. What was very strong and vigorous at four sheets had apparently almost disappeared at eight sheets. Now unless there is some other explanation, this would indicate that the rays can, as it were, go a certain distance and are stopped or diffused, or that certain rays are filtered out that with the lower thicknesses, gave the shadow; while those which were able to penetrate the greatest thicknesses kept right on and penetrated object and metal all together. But then a caution comes in here. It is possible that the back diffusion of rays from the operator not working back of a heavy metal shield is illuminating the screen by diffusion from the surrounding objects, and we may have an effect something like the opposing lights in a Bunsen photometer which, in a certain definite amount neutralize each other on the screen. Such is a possible explanation of the effects. There is needed much further experimenting.

It is curious to notice with the tubes having comparatively low vacua that no effect of production of rays occurs with the ordinary passage of a silent discharge, as by the Holtz machine, but the tubes often become good sources of rays if you put a condenser on the terminals and use a spark gap. A tube which is absolutely of no use for a steady discharge from the terminals may be made oftentimes very active by this simple expedient.

We often come across very curious things in this work with different forms of tubes under different conditions, and I think there is hardly a more fascinating field than working with these vacuum tube arrangements. There are no two alike. You can hardly produce the same exact effects twice. I may mention a curious thing which I noticed the other day. We had a spherical bulb with a wedge of platinum, the cathodes opposite each other. We made one of these actual cathodes, and the platinum the anode, with commutated current excitation. The vacuum in the tube was a little low, and one would suppose that the side of the platinum nearest the real cathode would get hot, but I was astonished to find that there was a bright spot on the other side opposite the idle cathode and none on the side opposite the real

cathode. This latter side was not even hot, but the apparently idle cup on the other side of the tube had sent out something or other which produced a red hot spot opposite to it and gave rays. That is only one of the curious things we find, which among the many other serve to mix us up and carry us perhaps farther away from the real thing that we all are looking for.

In regard to the diffusion of the rays, I have to say a word or two. Some time ago I tried to start some experiments in carrying on the work of investigating diffusion of Röntgen a little more fully with an object in view. My idea is represented in about this way. (Illustrating): This represents a vertical metal screen M. Fig. 2. I placed a Crookes tube here at T. I put a block of paraffin here at P that I can turn about. I put a heavy metal screen at the back at N. I placed a fluorescent screen here at F, facing in the direction of the arrow and shield-

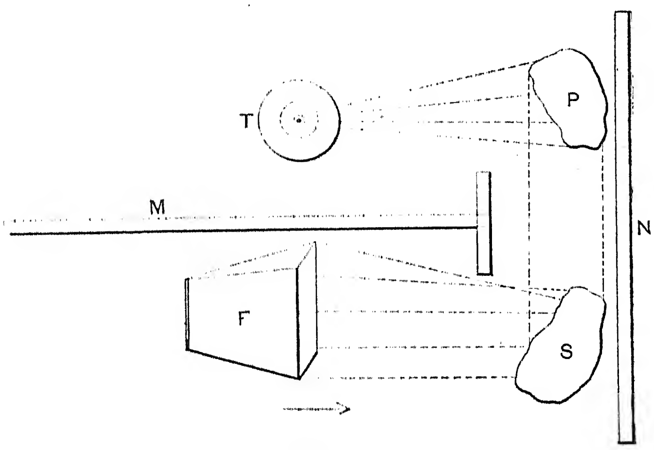


FIG. 2.

ing it in every possible way by metal. If then I get from this paraffin P, diffusion (which I know I can get from back, sides or anywhere) it simply behaves like a lump of opal substance, and I have the screen F in position such that no ray could reach it from P; then if I put another block of paraffin here at S and I get rays from it back to F I have what I may call secondary diffusion. If I then get the same effect from S as from P it indicates that the rays from P are Röntgen rays, and answers the very point that Prof. Rowland raised in regard to diffusion. They would be Röntgen rays, and again doing the same thing that they did at first at P, received at S and sent out again by the secondary diffusion. I tried some experiments in this direction, but I found my screens were not thick enough. I thought I got some effect, but the work will have to be done over again.

There was another point in this connection which interested me

very much and will require a good deal of time in experimenting. I was going to put a substance like paraffin here at P and say wood there at S and then interchange them. Now, if we do this, and do it enough times and with many different substances, we may be able to—especially if we could measure quantitatively the relation or effect—to discover that there is a spectrum or color value for each substance. If this at P, for example, absorbs certain wave lengths and sends out others, and this substance at S being the same substance, passes them on, and then is replaced by another substance which has a different Röntgen ray color, absorbing more of the diffused rays, the screen would be darker. Interchanging the positions and changing the substances to any length would enable us at last to get at the spectrum, if there was any such thing, and at the same time to say that paraffin had a certain ray color, so to speak, and so on for other substances. This is in the future. I simply mention it now, wishing that somebody had more time than I have to investigate this field and find out what there is in these speculations.

I was interested some time ago in regard to the effect of Röntgen rays on the tissues. I had read a few times that certain people had been burned by Röntgen rays. I did not believe it. These rays went through tissue so easily that their action could not amount to anything, but it was certainly worth while investigating so as to know. So I used a tube, which happened to be a heavy blue glass tube with a clear glass window. The blue glass did not allow the rays to get out, and they were absorbed except through the clear glass portion, where I wanted them for use. I put my finger up to the clear glass window and kept the other fingers pretty well shielded by the blue glass of other parts of the tube. I exposed the finger for half an hour to the rays, a Holtz machine being the source of electricity. I put the finger up pretty close to the tube, and after half an hour I thought that perhaps it was not long enough; perhaps it was not half enough. But if there were to be any effect it would be equivalent to a few hours distance, and as I got tired I went no farther. I shut down the tube and went away. Five, six, seven, eight days passed and nothing happened, and I felt that people had been mistaken about the effect of the rays. But on the ninth day the finger began to redden; on the twelfth day there was a blister, and a very sore blister. On the thirteenth or fourteenth day after exposure, the blister had included all the skin down to the part not exposed and had gone around the finger almost to the other side. The whole of the epidermis came away and left an ulcer without any possibility of recovering its own epidermis except from the edges, and I had to go through that painful process of having a raw sore there and the epidermis growing in from the side and gradually closing up. Only three days ago was the sore actually closed, and the skin is yet very tender, and nature does not appear to have found out how to make a good skin over that

finger. The skin still comes off in flakes and is very disagreeable and very tender, and there is a burning, smarting sensation every now and then. But I am satisfied it is coming out all right. I showed the finger, when at its worst, to my family physician. He looked at it and said: "Do you think you are going to lose that finger?" I said: "No; I don't think it is as bad as that;" but I must say that for a time it was a very angry looking affair. Some one has said that this is an electrostatic effect; the rays could not do that; you have got an electric burn, and they are notably difficult to heal. This has taken six and a half weeks to-day. But that view, I think, is negatived by a case reported, where a girl at Oberlin College was made the subject for examination through the chest, and her clothing was not removed, and yet she suffered from a very extensive, large ulcer and had to go to the hospital and stay there for treatment for quite a while. The doctor who had charge of the case told me of it and said it was a very angry looking sore indeed, and the astonishing thing was that it was accomplished through her clothing. Now if it had been electrostatic or ozonic, or in accordance with other theories that have been put forward to account for it, I don't think it would have occurred through the clothing. It must have been the radiation; but whether it is the radiation of Röntgen or some other kind of radiation is another question, and that is still open. It seems to me that what is likely to produce it is the lower rays; not the ones that go through metal but those of the lowest order, and this seems to be indicated by the fact that it stopped on the edges of the finger and did not reach the other surface. If it had been the rays that go through, I think it would have been the whole finger that would have been affected. It possibly has been a selection out of the lower rays, and if in examinations we could screen those off, perhaps we would have no trouble in this way.

I must tell you a rather amusing incident in this connection which is somewhat of a joke on myself. We had a mouse caught about a week ago. I thought, now, here is a good subject; perhaps I can take the hair off him. So I put him in a very small wooden box with sides about an eighth of an inch thick and exposed him for an hour to a very intense source of rays through the wood of the box. I put the mouse away in another box—I did not want him to run around in the first and get out of the sphere of influence—I put him in another box and had a little glazed cover so he could get air and I could see him inside. I put him on a pretty high shelf in the hall at the house. One night I was writing at my desk at about eleven o'clock when I heard a noise that made me think the clock was getting ready to strike. As soon as I got ready, I went out and there was the cat on the shelf—and the mouse gone. If we ever see a hairless mouse around the house after this we will know what was the cause of it.

I do not know that there is very much else in this connection

that I can say. I did not intend to speak about theory, and I will give place to others. If anything occurs to me later, and there is an opportunity, I may add something.

Dr. M. I. Pupin :—After a discussion of this subject by two such men as Professor Rowland and Professor Thomson, it is difficult to add anything. They have certainly said a great deal about it. They both confessed their ignorance of the subject, and five minutes ago I wondered how much more they would have said if they had known something about it. A few more words only can be added concerning the experimental side and still fewer concerning the theory.

When the discovery was first announced here, I, of course, was just as anxious as everybody else was to make myself familiar with the new radiation. But I had neither an induction coil of an effective size nor Crookes tubes, and so I used the high-frequency coil and vacuum tubes without internal electrodes; the results obtained were good enough for that time and I was perfectly happy. Professor Lodge confirmed my work regarding the possibility of obtaining Röntgen rays by tubes without internal electrodes. The experience which I obtained with the high-frequency coil was not a pleasant one, because it used the tubes up too rapidly. In fact, after exhausting my stock of electrodeless tubes I obtained some genuine Crookes tubes, and managed to break most of them with the high-frequency coil. Then I thought I would change my generator for the sake of saving tubes; so I borrowed a six-plate Holtz machine and some Crookes tubes from Professor Doremus of the College of the City of New York and obtained rather good results in this way. In fact at that time I thought that the Holtz machine was very much better than anything else. I had not yet tried a good induction coil. But even now I agree perfectly with Professor Thomson, that the influence machine of large output would be an ideal machine, only it is too expensive a piece of apparatus and perhaps somewhat too bulky. An induction coil proved, so far as my experience goes, more satisfactory than anything else—an induction coil of large output—so that one can always have reserve power and can call upon it when he may choose. Now it seems to me that in the use of the induction coil there is a great deal of difference of opinion as to the modus operandi. I have used as many as 120 breaks per second, and others have used 200 breaks per second; and I have used also a rotary current interrupter, thinking—and I believe my opinion was correct—that a rotary current interrupter would avoid sparking, and give a much cleaner break than a vibrating current interrupter. Yet I have seen most creditable work done by ordinary vibrating interrupters, where the number of interruptions was not more than 10 to 15 per second, or even less. *I am not very sure that it is quite clear why a small number of breaks can evidently produce just as good effects as a large*

number of breaks, both employing the same spark-length. This seems to me to be the most important practical point to investigate, because it is by the solution of the problem of producing the X-rays in the most efficient way that we can expect to solve a great many other pending problems, like, for instance, the attempt which Professor Thomson described here for increasing the wave-length by successive diffusions. In this case the chief difficulty would be that we would have to use substances like paraffin, wood and so forth for the diffusion effects, because as I demonstrated last spring[1], insulators, as a rule, especially wood and paraffin and so on, are the best diffusers, but at the same time the best transmitters of the X-rays, so that only a small proportion of the X-rays are diffused by the substance, and if you use successive diffusions there will be after three or four diffusions very little left to experiment with. Hence the necessity of producing the X-rays in as large quantities as possible. Now there

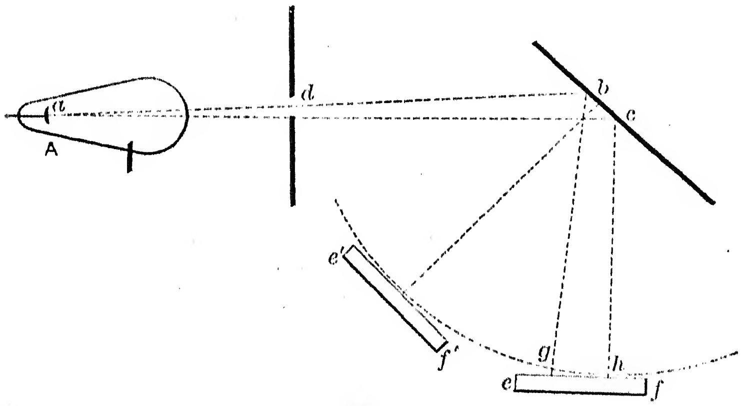


FIG. 3.

is no doubt that a certain quantity of energy per second must be sent through the Crookes tube in order to produce the best effect, and the question arises whether we are to follow what may be called the homeopathic or the allopathic way of sending that energy through the tube. That is to say, you can send 5 watts in doses of one one-hundredth part by 100 breaks per second, or we can send these 5 watts in doses of one part at a time by having only 5 breaks, and then the question arises, in which case will we get the best effect? Preliminary experiments that I have made so far (but I have not done much lately on account of illness) seem to show that up to a certain limit the best way is to use large quantities of energy at large intervals and not small quantities repeated in rapid succession.

Now as to diffusion and specular reflection of the X-rays.

1. *Science*, April 10, 1896.

Last spring I was trying to produce a reflection of the X-rays at about the same time other experimenters were doing the same thing, both in this country and abroad. Some of these men obtained what they supposed to be regular reflection. The method of procedure, as you all know, was to place a polished reflecting plate at a certain angle to the X-rays, and then place perpendicularly to the direction in which we would expect the X-ray beam to be reflected, a photographic plate, and if an image is obtained, that would be some sort of a proof that the X-rays were reflected. At that time it struck me that this does not constitute a valid proof that the X-rays are reflected, since diffusion would produce a similar effect. In all experiments of this kind, the effect of regular reflection will be very much masked by the effect of diffuse reflection, and now permit me to say a word or two on this point and also to suggest a way in which this question may perhaps be decided. Suppose that we have a tube at A, Fig. 3, so that the X-rays will proceed along *a b* and *a c*. Say that we have a slit at *d* and allow only the part of the X-rays which go through this slit to strike a polished surface *b c*. Then if we place a photographic plate at *e f*, say near to the angle in which we expect regular reflection, we should obtain an image of this slit at *g h*, that is to say, the maximum density of the radiation received by the plate *e f* will be somewhere between *g* and *h*. But we might obtain a similar maximum even if regular reflection does not exist; for if the strip *b c* is diffusing perfectly, that is, somewhat as an illuminated wall diffuses light, then the maximum amount of diffusion will be in a direction perpendicular to the plate *b c*, and the amount of diffusion in any other direction will be equal to this maximum multiplied by the cosine of the angle included between this direction and the normal to *b c*; in other words, the diffusion follows Lambert's law. Now a simple consideration will show that the maximum amount will be received by a strip along *g h* where we get a maximum by regular reflection; so that the maximum at *g h* does not by any means indicate that there is regular reflection; because, as I said, diffusion will also give a maximum *there*. Now, suppose that both diffusion and regular reflection exist; then the two effects would be superposed there, and we would not know how much is due to regular reflection and how much is due to diffusion. If, however, we use at *b c* instead of a plane reflecting plate, a spherical mirror *b*, *c*, *d*, Fig. 4, and allow the X-rays *a b*, *a d* to fall on this spherical plate, then the part which is diffused will be brought to a focus at the centre *e* of this mirror, because a radiation by diffusion is always maximum normally to the elements of the surface, that is, if it is diffusion like the ordinary diffusion, and that it seems to be according to my experiments; whereas the rays that are regularly reflected would be brought to a focus at a different place, somewhere in the vicinity of the point *f*. In that way we could separate the two, and also by measuring the time

of exposure at the points c and f get approximately the comparative strength of the two reflections.

The subject is sufficiently interesting to deserve a careful experimentation of this kind, although, of course, it would be very difficult to produce desirable effects, because if we use a polished mirror like a speculum-metal mirror and allow only a small amount of X-rays to fall on it, reflected radiation is extremely small and the time of exposure would have to be excessively long. Still I think that the effects can be produced, and I intend, as soon as my health will permit, to make experiments of this kind.

Now as to the theory. With your kind permission, I venture to add a few words to what Professor Rowland has already said, and wish to call particular attention to some of Professor Helmholtz's theories. This is the more desirable as one of these theories has been criticised lately rather harshly

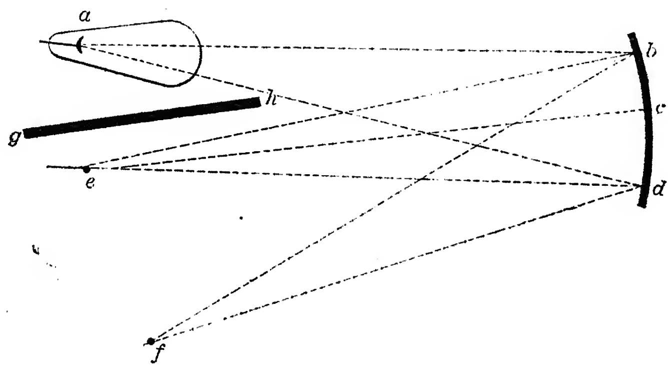


FIG. 4.

by Mr. Oliver Heaviside, owing undoubtedly to a misunderstanding on his part. Among the various theories suggested last spring to account for this X-ray radiation there was one which Professor Rowland did not mention, but which I shall briefly describe, because it resembles somewhat closely the theory proposed by Professor Stokes: namely, the theory that in the X-ray radiation we may probably have a sort of circulating motion of the ether. You know that according to Maxwell's electro-dynamics the stresses in the electromagnetic field are distributed in such a way as to produce a pondermotive force acting on the ether, and this pondermotive force is, as a rule, counterbalanced by the incompressibility of the ether; so that, as a rule, there is no motion of the ether under the influence of this force. For instance, a rubber ball when it is compressed by a uniformly distributed normal surface pressure, counterbalances this pressure by its elastic reaction and there is no motion of the rubber ball as a whole. Helmholtz was the first to propose

the question whether it is possible that the ether should move bodily, owing to the action of pondermotive forces not balanced by its incompressibility. It is not necessary to go into any detailed discussion of what would happen in the ordinary magnetic field. I will only mention the case which Helmholtz discussed, namely, the case where there is no ponderable matter in the ether. Suppose that we have a space containing no ponderable matter; the question arises then, can there be such a distribution of stresses, or in other words, of electric and magnetic forces, as to produce a motion of the ether there. Helmholtz gave an answer to this question and proposed—this was in 1893—to go into an additional discussion of the same problem, but unfortunately he died in 1894. This is the answer which he gave: Suppose that there is motion of electromagnetic energy; then say that the velocity of this motion in a given direction is p, then the pondermotive force acting upon the ether in that direction is proportional to $\frac{dp}{dt}$. This force is not counterbalanced by the incompressibility of the ether and therefore will produce motion of the ether.

Let us suppose that this space is a Crookes tube. We have then an approximation to a part of an electromagnetic field in which there is no ponderable matter. Let us accept the hypothesis which is so well supported in England, namely, that the cathode rays consist of negatively charged particles, moving at very high velocity, we shall have then as soon as the spark breaks through between cathode and anode, and negatively charged particles begin to move from the cathode, a motion of electromagnetic energy, and it can be easily calculated, and was calculated for a single charged particle by Professor J. J. Thomson in the first chapter of his book on "Recent Researches in Electricity and Magnetism." The motion of that electromagnetic energy is in the same direction in which the particles move. The lines of force of the magnetic field produced by a motion of a charged particle are meridian circles to which this line of motion is an axis. Lines of electric force are everywhere perpendicular to these circles. Energy moves along lines which are perpendicular to the electric and magnetic lines of force. We have then, when a negatively charged particle begins to move from the cathode, a beginning of motion of energy—the velocity changes from zero to a certain very large quantity. Therefore we ought to get, as soon as the discharge commences, a pondermotive force which tends to move the ether in the direction of the cathode rays, and this force will be the more intense the more disruptive the discharge is. When the negatively charged particle strikes an obstacle, say the glass wall of the vacuum tube or the anticathode, we should have then the energy flux changing from a very large value to a certain very small value. We get then again a force tending to move

the ether in the direction directly opposite to the cathode rays. According to this view the cathode and the surface struck by the cathode rays become the seat of points from which radiate forces tending to produce a bodily motion of ether. Whether they really produce an actual motion of the ether or not, and what the nature of this motion is—these are questions which can be decided by the coördination of experimental facts derived from the study of the X-rays. This possible view of the X-ray phenomena was suggested by me in an article which I published in *Science* of April 10th, 1896, and I still consider it worthy of some consideration, although I must admit frankly that in our present state of knowledge of the physics of X-rays, very little weight only can be attached to any theory.

There is another view which I wish to discuss briefly, and that is the view offered by Helmholtz's electromagnetic dispersion theory. You have heard it mentioned that the X-rays are in all probability ordinary light of very short wave-length. Then, according to Helmholtz's dispersion theory, these wave-lengths may be so short as to suffer neither refraction nor polarization by ordinary matter. Let us see whether this theory explains how we should be able to produce these rays of excessively short wave-length by the electric action in a vacuum tube. Every dispersion theory which explains how a substance will act with respect to certain waves, must also at the same time be capable of explaining how these wave-lengths can be produced. Otherwise it does not completely fulfil its function. The Helmholtzian theory differs, in my opinion, radically from all other electromagnetic theories of dispersion in this way: Some of these theories proceed from no definite physical basis, but start with tentative mathematical assumptions. The physical basis of the others may be obtained somewhat as follows: Suppose that we have an electric system consisting of say seven parts, each part containing self-induction, capacity and ohmic resistance. This system of electric conductors is a vibratory system, and if disturbed by an electric impulse it would become the seat of electric vibrations, not vibrations of a single period, but of seven different periods. That is to say, in each of the seven conductors we should have an electric oscillation composed of seven singly periodic vibrations. The amplitude of each particular frequency in any conductor depending, of course, on the constants of that conductor. A system like that when disturbed, will send forth electrical waves of seven different wave-lengths, and if we could use suitable means like prisms, we should get if the waves are sufficiently short; an electric spectrum, because of course, for each of the seven different waves the prisms will have a different index of refraction. Now suppose that we diminish the size of this electrical system until we get to molecular dimension, we would then have what may be considered a molecule consisting of the component atoms, each atom, as it

were, having all the electrical properties of an electric conductor, namely, self-induction, resistance and capacity. Each molecule would then have a multiple period of vibration; and therefore capable of sending so many different wave-lengths, which sent through a prism, would give a spectrum of the molecule.

Now several of the more important dispersion theories to-day, are based on physical conceptions of this kind—that the molecules consist of component parts, and each component atom is in a certain sense an electric resonator, having the properties of self-induction, capacity and resistance. This assumption forms the weak point of these theories. Now the Helmholtzian theory is radically different in that it makes no assumptions of this improbable kind. The physical basis of the Helmholtzian theory is suggested by the experimental fact which is the foundation of modern electro-chemistry, the fact namely, that to every valency of an atom, there is a definite quantity of electricity attached, and that therefore in a molecule we have in each component atom a perfectly definite quantity of electricity. Then, according to Helmholtz's theory, if there is electric force in the ether, say an electric wave, that electric wave will act upon the molecule by acting upon the electrical charges which are attached to the valencies of each component atom. Such a molecule when disturbed will become the seat of electric vibrations, not vibrations such as take place in an electric circuit, but vibrations which we get by causing a charged body to oscillate. This kind of electric vibration in the molecule is determined not only by the electric relations between the component atoms, but also by the mass of the atom, and also by the mechanical force acting between the atoms in the molecule. Helmholtz's theory discusses these vibrations in a bipolar molecule, but it is not at all difficult to see what would take place in a more complex molecule. A molecule consisting of say a thousand atoms would have a thousand different periods of oscillation. In fact each atom in the molecule vibrates with a complex harmonic vibration, consisting of the superposition of a thousand simple harmonic vibrations of different periods. Some of them may be very high and some very low, comparatively speaking; and whenever an electric wave of a periodicity corresponding to one of these periodicities strikes the molecule, there is resonance; that is, the wave is very much absorbed. The index of refraction of a wave passing through a substance having resonating molecules, will be quite different than the index of refraction for waves which awake no resonance in the molecule, since both the index of refraction and the coefficient of absorption for a given wave depend on the resonance relation between the wave and the molecule.

This short account of the dispersion theory suffices to prepare us for an explanation of the manner in which these very high frequencies and the accompanying excessively short waves which

would correspond to the Röntgen rays could be generated by the discharge in a vacuum tube. Consider a vibratory system consisting of a thousand different but interconnected parts. It has a thousand different periods of vibration, and each component part will vibrate with the same complex harmonic vibration consisting of a thousand simple harmonic components. The relative values of the amplitudes of these simple harmonic components depend altogether on the manner of excitation. Take a bell, for instance. If you strike a bell with a small hammer and give it a very sharp tap you will bring out the high shrill notes in the bell. That is the higher vibrations will be brought out more prominently than the lower ones. In the same way if a molecule is excited by striking the molecules against each other, as occurs during the molecular motion which corresponds to sensible heat, then the higher the temperature, the quicker, the sharper will be the tap, the stronger will be the higher vibrations. This is the generally accepted explanation for the shifting of the spectrum energy toward the blue end as the temperature increases. In the Crookes tube we in all probability have the best method of exciting the very highest vibrations; because here the negatively charged particles which are torn off from the cathode are moving with enormous velocity, and wherever they strike an obstacle, say the glass of the tube or the platinum plate of the anode, there they produce an impulse of the very highest degree of sharpness, very much sharper than any existing temperature has yet been able to produce. The amplitudes of the very highest vibrations in the molecule are made prominent, and they manifest themselves as the X-rays. But if the X-rays are really transverse vibrations, then we should in all probability along with them have other vibrations of lower period also, but only weaker, just as in the ordinary excitation by means of the electric arc or by a Bunsen burner the amplitudes of the lower vibrations are brought out prominently, but the higher vibrations are also present but only much weaker.

Helmholtz's theory was criticised lately by Mr. Oliver Heaviside, but unjustly, owing undoubtedly to a misunderstanding on Mr. Heaviside's part. Mr. Heaviside applies a peculiar test to this theory; the test, namely that the system of mechanically vibrating iöns should satisfy certain conditions which exist in an electrical system composed of coils and condensers, such as I have described above. If we examine a little more closely we will see that the Helmholtzian theory, although it is not called upon to satisfy conditions of this kind, does actually satisfy them, provided, however, that a single mistake, which was corrected a year ago by Dr. Reiff, is corrected. This mistake does not affect the main results of the Helmholtzian theory, and there was no necessity to worry so much over it as Mr. Heaviside did, especially when there is danger that his criticism might produce the impression that the Helmholtzian theory, which was brought out

very prominently in connection with the X-ray phenomena, might have some weak and inconsistent point in it, and therefore might be misleading. It is not so, but on the contrary it is one of the most beautiful and most suggestive theories in this connection.

Dr. A. E. Kennelly:—Mr. President and Gentlemen:—I have been so much fascinated with the delightful remarks we have heard this evening from some of the earlier speakers, that I only regret that in a weak moment I consented to join in this discussion, and that I have so little to add to it.

Just one word about the theory of the subject. Whatever may be the nature of the Röntgen ray, it must be either a bodily movement of matter, or a movement of a disturbance in matter, or a bodily movement of ether; or, finally, a movement of a disturbance in ether. As regards the first supposition, that X-rays are streams of matter in motion, or of projected molecules, there is an experiment which seems to negative it. A Crookes radiometer vane does not, as far as I have been able to discover, recede from an excited Röntgen ray tube. One would suppose that it would be powerfully repelled, if X-rays were bombarding streams of particles.

As regards the three remaining hypotheses, it is to be hoped that future researches will show that X-rays are ultra-ultra-violet light rays. Not only would this be the simplest conception that we can at present frame, since it would only call for further extension of the spectrum, but it would also keep X-rays within the limits of electro-magnetic waves and in the immediate province of electricians.

In reference to the applications of X-rays, I have made a few experiments, in conjunction with Professor Houston, upon the perception of X-rays by the blind. These experiments seem to show that where the mechanism of the retina has been destroyed, leaving the optic nerve in a useless or atrophic condition, no X-rays are perceived. When the mechanism of the eye is intact, but the optic nerve is deranged or paralyzed, some visual conception may be obtained by the stimulus of X-rays. When the optic nerve and retina are both intact but the cornea is deranged, the fluorescent effect of X-rays upon a calcium tungstate screen held before the eyes, excites the visual sensation in the ordinary manner to a large degree that depends upon the corneal opacity. It would seem, however, although it is not certain, that the corneal opacity may itself feebly serve as a fluorescent screen, and that X-rays filtered through wood or pasteboard falling on some eyes that are corneally blind produce a faint visual sensation of diffused light.

Mr. Max Osterberg:—I have only a few words to add, and those words are practically in connection with remarks made by Prof. Thomson and afterwards discussed by Dr. Pupin. They are in regard to the amount of energy necessary, and furthermore in

connection with the kind of vibrator used on the induction coil. We might add just one more condition and then the subject will be a little plainer. Let us differentiate between an investigation which we want to perform by means of taking a picture, and one where we simply wish to make an investigation with a fluoroscope. Of course, I, like the others, have generally used my bones, because they were in most cases handier than coins. I always had them with me. The question of energy, to my mind, is simply this: In the first place we want the energy to be in the primary circuit of our induction coil. In the second place we want this energy in the secondary of the induction coil. In order to get it into the primary, it is necessary that we have a large induction coil; that we should charge the induction coil for quite some time. In other words, the make should be rather long, while the break should be very disruptive. We cannot possibly produce a very long, or comparatively long make with a very short break on an ordinary vibrator; and consequently we are forced to use a rotary circuit-breaker where we can make the length of charge just as great as we choose. So far as the breaking is concerned, it appears that the most essential point is that this break should be disruptive. In a fluoroscope investigation this would hurt the eye very badly, because there would be constant flickering. I have tried several times to close the circuit of the primary for some time and then break very suddenly. I found at those times that the fluoroscope would fluoresce considerably—in fact, a great deal more than it would if I kept the rotary vibrator or the ordinary vibrator on the circuit. But, of course, if you want to take a picture, and if it is true that we only get the real X-ray effect at the moment of break, then it is practically a question of length of time of exposure. If the ordinary vibrator gives us a considerable effect, we might as well have the ordinary vibrator and expose a greater length of time. But if we can put more energy into the primary circuit with a rotary vibrator, we might better do that.

As to the energy in the secondary, it seems to me that it should be properly distributed. There are three places through which the energy in the secondary of the induction coil is illustrated: First, in the secondary of the coil, then in the leading wires, and finally in the tube. It is found that unless the tube works at its very best condition for the production of these rays that there is considerable brush discharge along the wires. This cannot be regulated very well because we do not know how to regulate the tube; but it can be regulated by changing the leading wires. I think Dr. Pupin was the first one to suggest that, and I believe he must have been arguing in the same direction when he made the suggestion, and that is to introduce an air-gap in the leading wires from the secondary to the tubes. This air-gap then will, of course, be one of the parts of the circuit, and that can be varied from a very small fraction of an inch to

quite a considerable length. In that case the brush discharge is practically nothing, and the tube can be constantly kept at a certain constant fluorescence of activity.

The next question is that of the alternating current *versus* the direct. I meant to say something on that subject, but Prof. Thomson has cut me short somewhat, because he outlined or rather explained the new method which had been suggested by Mr. Lemp and employed by him; that is to use a direct current or uni-directional current in the tubes. Of course, ordinarily this has not been done, but I think that up to the present, the alternating current would be very much inferior to the direct, for the simple reason that the anode and cathode being constantly changing, there will be a constant disintegration of the platinum as soon as it becomes the cathode, and this will tend to blacken the bulb, and therefore spoil the tube rather quickly. The direct current, furthermore, will be constantly acting in the same direction, while if the alternating current acts twice, the frequency would practically correspond only to one-half the number of makes and breaks on the direct circuit, and in that way the time of charging the primary coil would be doubled by using the direct current, or it would only be half the amount by using the alternating current.

Prof. Rowland:—I made a few notes with regard to what has been said, but they are made in such a way that I do not believe that I can interpret them myself, especially as the hour seems to be getting rather late. One or two remarks, however, I would like to make. When Prof. Thomson said that he got such a large amount of rays from an insulated piece of platinum by letting the cathode rays fall upon it, he made a sketch (Fig. 1). With the exception of this end, which was flat, that is the kind of tube that I used. Now, there was absolutely no effect when *this* was made an anode and *this* a cathode, so that all the cathode rays were striking on the platinum. I have the photograph; I got no effect whatever. Now, if Prof. Thomson got an effect in *this* case and I did not get an effect in *that* case, I have got a case, at least, where none of these rays were produced by the fallng of the cathode rays upon the object. It doesn't make any difference how many other persons have something in which they do get an effect. If I did not get an effect, that is one case, understand. That is the case where the cathode ray fell on an object and I got no Röntgen ray. If other people got them in other ways, why, there is some other disturbance coming in. I don't know what it is.

Prof. Thomson:—I should like to say just there, professor, if you would allow me, that I used exactly that arrangement first, and got rays with the concave cathode. The anode at this end and the interposed plate of platinum between, with that wire extending outward, is the standard form of Crookes tube—the first tube, in fact, that I used. I got not only sharp effects but rays.

The Chairman:—Was the platinum red?

Prof. Thomson:—The platinum was red—yes, of course, and it was a vigorous source of rays. I got rays with the same tube that Professor Rowland does not get them.

Prof. Rowland:—Well, that has nothing to do with the point. The point that I raise is this, that there was certainly no doubt that I did not get any, and the cathode rays were falling from the object. That is the thing. Now, one thing that I wish to remark is that most people draw a tube like that. They don't say where the wires go. Mine generally went out, so that they were very far away from this object. By curving wires around in different ways I can get an inductive action. I don't doubt that I could fix up a tube so that I could get lots of rays out of any part. However, the time is passing, and I will just say one word with regard to the point Prof. Thomson raised with regard to the fluorescence over the surface of the glass. He thought something was stopped by the glass. I must say that Lenard, when he first experimented upon this subject—and I regard his experiments as quite as valuable as Röntgen's, probably—, he got several kinds of rays coming out through an aluminium window. He got rays which were deflected by the magnet, as well as others. He had not separated them however. When the Lenard paper came to the laboratory I remarked to my students: "That is the best discovery that has been made in many a day." I immediately set somebody to work experimenting. He tried to get some results and would probably have discovered the Röntgen rays at that time if it had not been that the University of Chicago called him off, and Johns Hopkins University was very poor and could not call him back, and he had to stop in the midst of his work. They always say in Baltimore that no man in that city should die without leaving something to Johns Hopkins. Now, Dr. Pupin mentioned a means of showing whether the rays were reflected—a little reflector in which he had them brought to a focus, as I recollect it. I have read an account in which an experimenter did find the rays were brought to a focus, showing, provisionally at least, that there was some regular reflection. But these experiments should all be repeated many times before one actually believes them. We don't always believe what we read.

Now, as to Helmholtz's theory of the motion of the ether and so on—well, as I said before, what is the motion of the ether? What is motion of the whole ether? You cannot move the ether in the whole universe all at once, and if you do not move the ether in the whole universe all at once but only move a part, then it is a wave, so it amounts to the theory that I gave—an impulse, such as Stokes had. Now, an impulse such as Stokes had does not go in a straight line—it goes around corners—and it does not go in a straight line unless there are lots of waves coming out. We can readily prove that an ordinary molecule, vibrating to ordinary light, must give out a hundred thousand

waves without much diminution of amplitude, or else you cannot have the sharp lines in the spectrum that we do. The molecule must vibrate a long time—longer than any bell that we can make. We cannot find a bell that will give out a hundred thousand vibrations without much diminution. For etherial waves something must vibrate to produce them. What it is I don't know that there is any necessity for discussing, because you can discuss it forever and never get any nearer to it. Something vibrates. Now the thing that vibrates we don't know. We don't know whether it is electricity or whether it is mechanical motion. We know nothing about it. I have often said to my students, when I showed them the spectrum of some substance like uranium, in which we were taking photographs which would be perhaps ten feet long—so fine in grain that you could not put the point of a pencil on it without finding a line. There were thousands of lines. I said to them: "A molecule of matter is more complicated a great deal than a piano. Counting the overtones and everything, you would not probably get up anywhere near the number of tones you get out of a single molecule of uranium. Therefore it rather looks as if the uranium molecule was very complicated." Of course, all those spectrum lines do not indicate fundamental tones—many are harmonics. Still it is rather a complicated thing to get a spectrum in which there are many thousands of lines. So when I come to think what a molecule is and try to get up some theory of it, I quite agree with Dr. Pupin that we don't know anything about it.

PROF. W. M. STINE:—(*Communicated.*)—In the early stages of these investigations, we were compelled to work by conceptions of the nature and conditions for the production of Röntgen rays, based largely upon *a priori* reasoning, and the contradictory views held to explain the varied phenomena of low vacuum tubes under electrical excitation. The battle of opinions which has waged almost ceaselessly since then, has left the question very nearly in the same conditions it was received from Röntgen and his immediate predecessors. That one, is both ignorant and careless who affirms that no progress has been made, no additions added to Röntgen's observations; but there is no greater definiteness of views concerning the nature of these newly discovered rays. The only gain here has accrued from the process of exclusion; though we do not know what the ray is, we have learned that it is not to be classed with a few weird light phenomena. The battle of the physicists has been a curious struggle; but, as usual in the history of science, prejuduce, prepossession and conservatism made a vigorous showing, and have scarcely yet realized their defeat. Again, as has often been shown, many scientists had recourse to evolution from their inner consciousness, fled to man-built conceptions of how the universe was con-

structed, followed the leadership, in spirit, of Aristotle, instead of emulating Faraday, in a direct appeal to nature. Never has the intellectual world been presented with a clearer evidence of the futility of *theoretical* systems of natural philosophy. It has been forgotten that Maxwell succeeded Faraday, and that his self-confessed merit was that he was enabled to give exact quantitative expressions for those physical facts and relations which owed their discovery to that splendid genius which had so successfully questioned nature.

It is in the spirit of this direct appeal to nature that the attempt is here made to present results of experiment and observation, and they are offered for the reason that they have received such uniform confirmation. They can probably be best presented by studying the tube historically. The results here stated were obtained from both foreign and domestic tubes, including a great variety, both in size and design.

It will be assumed that the tubes are in all cases excited from an induction coil, in which the condenser has been adjusted to best conditions of resonance, and that a continuous current is employed, interrupted at least 50 times per second.

The tube, as received from the maker, will produce the ray vigorously for a time, but if the excitation be very powerful, it will shortly break down and cease to emit the ray. What is observed during the break-down is a bluish purple halo which makes its appearance at the cathode, and a halo at the anode as well, though this is grayish in tint. Gradually the blue halo lengthens out into a pencil, extending from the centre of the cathode towards the impact surface, and in a short time the pencil extends completely from the cathode to the impact plane. As soon as the blue line is seen to do this, the tube fails to emit the ray when viewed through the fluoroscope. Following this, the tube slowly fills with a bluish light to a greater or less extent. This "dead" period of formation will last for hours, the time being usually shortest in such tubes as are provided with a large anode surface, in addition to the metallic impact plane.[1] As the "formation" reaches completion, the fluorescence of the tube becomes a brilliant greenish-yellow. The halo about the anode disappears from its face, and is seen as a faint blue light in its rear; the pencil of blue light has also disappeared, though there may still be a faint halo in front of the cathode. During this period the resistance of the tube has greatly increased, it having fallen to a low value during the break-down. The fluoroscope now glows brilliantly, but the rays have scarcely any penetrating power—a hand placed in front shows only in outline, scarcely any details of the skeleton being visible.

A phenomenon is now marked in the focus tube, which persists throughout its life, and may be termed the impact plane of

1. "Röntgen Ray Tubes," by the author, *Electrical World*, Oct. 3, 1896, p. 383.

the ray. This is very clearly defined on the walls of the tube, and sharply marked on the screen of the fluoroscope. Its very sharpness of outline is an extremely significant fact which has so far received only the barest mention. Further than this, the rays given off in this plane exceed in intensity those given off along any other plane, and if we differentiate penetrating power from mere intensity, this property is also most marked in the impact plane; but until satisfactory radiometric methods are devised it is useless to attempt to state any ratios.

As the tube is continued in use it enhances, both in intensity of ray and penetrating power, the cathode and anode halos growing continually less perceptible.

After a time a period of decline sets in. The vacuum becomes extremely high, the fluorescence is scarcely perceptible, and the ray is weakly emitted, but its penetrating power has become so great that even the bony skeleton of the body becomes quite transparent. It is now customary at this stage to heat the tube with the flame of an alcohol lamp. As some erroneous statements have been made in this connection, the phenomena will be treated in detail. When the flame is first applied to the tube it darkens, being doubtless short-circuited by the aqueous vapor condensed on its surface. From time to time occasional flashes occur in the tube, but at length, with surprising suddenness, the entire bulb fills with the usual glow accompanying a highly active state, and the ray is emitted with great power. If at this juncture the heat is not very cautiously applied, the vacuum will be quickly lowered to the point of breaking-down; the impact plane will grow red hot, and the penetrating power of the ray is feeble. By deftly reversing the tube and manipulating the heating, the vacuum can be controlled within wide limits.

After a bulb reaches the point in use when heat must be applied, it ceases to be dependable, and its useful life is about over. Its uncertainty is greatly increased if the bulb has become blackened by a deposition of platinum, which rapidly occludes the remanant gases as they pass it in convection currents. To avoid this blackening, I have lately had the glass-blower substitute aluminium for the platinum of the impact plane. I have not found this change, so far, to affect the emission power of the tube, and besides, it lessens the cost. My experiments on comparative emission power of different substances have not been so extensive as could be desired; yet I have found that a surface of glass is best adapted for rays of the highest penetrating power.

In summarizing these experimental facts, is it not reasonable to assume that we may have some basis, some guidance, though slight, for an explanation of the Röntgen ray?

The first significant observation is the behavior of the tube as the blue pencil of light becomes evident. Vacuum tube experiments have demonstrated almost to a certainty that such phenomena are due to intermolecular collisions or impact; or, what

amounts to the same thing, that the mean free path of the molecules is short. As the vacuum lowers, this pencil grows in length and brightness, its molecular path, all the while, shortening. Conversely as the tube builds up, the molecular path increases, until at the moment the ray reappears, there is good evidence for believing it extends from the cathode to the impact surface. This, then, is the one prime condition for the production of the ray. At the same time, it seems equally clear that there is within the tube a certain critical pressure of its contained atmosphere, requisite for the generation of the ray. But the mean free path from cathode to impact plane, and the critical pressure, are correlative conditions. The suddenness with which the activity of the tube is resumed upon heating, is thus explained. The investigations of Rigi may be instanced in confirmation. He found that the critical pressure was some simple function of the distance between the cathode and impact plane.[1] But it has been found by many others that the shorter this path, the lower the working vacuum might be in the tube.

The second significant observation is the sharp definition of the impact plane on the walls of the tube and the screen, or exposed dry plate. This is indicative that the ray is generated at the surface of impact or in its immediate neighborhood. Experimental evidence all points to an impact surface for the production of the ray. If the focussing tube be accurately made, the principal impact is on the metallic anti-cathode; but whatever be the shape of the tube, there is more or less weak dispersion of the charged molecules of the cathode stream; and these impinging on almost the entire surface of the tube, constitute them a weak secondary source. Since the ray is produced only on impacted surfaces, and not in intermediate space, it follows that a mean free path is necessary to the impact surfaces.

Returning again to the principal impact surface, the usual pictorial representations of tubes are very inaccurate. The path of the cathode rays and the generated Röntgen rays is drawn as if it followed the law of incidence and reflection for transverse, or light waves. Here, again, is seen a further proof of the foregoing statements. That the law of incidence and reflection is not followed is because, at the surface of incidence, there occurs a transformation of energy resulting in the generation of a specific vibration.

Whatever be the substance of the impact surface, no true reflection occurs from it; and if it shuts off the rays in any direction, it does so by absorption. In this sense the material of the impact plane is without any influence on the distribution of the ray there generated. Another matter of interest is the change which occurs when the metallic impact grows red hot. In all my experiments, I have invariably found that the result is to

1. *Electrical World*, Sept. 26, 1896, p. 369.

lower the penetrating power of the rays. This may be due either to *decreased* amplitude or *increased* time of vibration, either or both being caused by the heating, which, driving off more occluded or impact gas molecules, and even the metallic molecules from the surface, lowers the vacuum; the electro-static capacity of the tube decreases, the voltage at its terminals lowers, and the molecules of gas are projected from the cathode with less velocity.

The question now arises, what is the hypothesis concerning the nature of these rays, which will best explain the observed facts here noted? The answer is beset on every hand with difficulties, but that which has best served to guide me has been one based on the vortex theory of atoms. But the difficulties here encountered are great. The vortex theory has only received a partial mathematical development, and scarcely anything has been so far possible towards physical demonstration.

In order to present these views, an assumption will be made— that the active molecules involved are vortex filaments. Whether such filaments are vortex rings of matter in the ether, or be simply ether in rotation, involves difficulties whose solution has scarcely begun. We will assume also the former condition.

If such a vortex filament is to be endowed with the power of emitting the range of transverse vibrations which the light and heat spectra demand, let it further be granted that it may emit a certain range of longitudinal vibrations. If the ray suffer collision without great change of radius, the increase of its energy will give rise to transverse vibrations. This will account for the halos and pencil of blue light. If it be granted that intermolecular collisions of gaseous molecules do not result in great changes of radii, we have an explanation for the necessity of a mean free path from the cathode to the impact plane, as a condition requisite for the generation of the ray. Against such supposition may be opposed the results of mathematical analysis

The effect of the impact of such a ring on a fixed surface is thus described by Lord Kelvin: "When a vortex ring is approaching a plane, large in comparison to the dimensions of the ring, * * * it begins to expand, * * * and will expand out along the surface, losing in speed as it does so."[2] There will be a period then when the impact or translational energy is all absorbed in increase of radius; then will begin a series of harmonic vibrations of radial length, assuming the ring is not deformed circularly from the impact. What will be the resulting strain imparted to the ether? Would not a vibration rate be impressed upon the ether, and would not this be longitudinal? This would only be possible in case the ether can suffer longitudinal displacement.

1. "Motion of Vortex Rings," J. J. Thomson, Part II; also Part I, for the vibrations of such rings.
2. *Nature*, Vol. XXIV., p. 47.

What has been stated of the vibratory elastic properties of such vortex rings with respect to transverse vibrations, can be applied to longitudinal movement, that "the vibrations are influenced by the type of displacement and their restitution force, involving constants of space, time and mass.[1]"

In the light of our present knowledge, such an hypothesis furnishes the only rational explanation of the influence of the hardness of the impact surface, conditions of excitation of the tube being the same, upon the penetrating power of the ray. For a given rate of translation of vortex rings, the less yielding the impact surface, the greater would be the radial increase, or, what amounts to the same thing, for a given vibration frequency, the amplitude would be greater.

The modes of attack on the vital question, whether such rays are transverse or longitudinal, have been indirect ones. The question can not be settled until appealed to some such crucial test as spectrum analysis furnishes for transverse vibrations. Who is the genius who will construct for us a grating fitted to analyze longitudinal spectra, as Professor Rowland has so beautifully done for transverse vibrations?

MR. C. O. MAILLOUX:—Before the audience disperses, I think it is only proper that we should emphasize the fact that we have been favored in a signal manner this evening in the discussion of the all-absorbing topic which bears such an important relation to the latest advance in the science of physics. This evening we have had worthy representatives of all the large cities within a radius of three or four hundred miles. We have, to begin by the perhaps best known and most favorably known, Prof. Rowland, who represents one of the leading universities of the world, as the representative of Baltimore; Dr. Kennelly from Philadelphia, and our own and only Prof. Elihu Thomson from Boston. I think that it is meet and proper that I should propose a vote of thanks to the distinguished—I will not say foreigners—but the distinguished members from afar—for they come to see us so seldom—for the honor and the pleasure which they have conferred upon us by coming to meet with us this evening to add the light of their experience and knowledge (theoretical and experimental) in connection with this important matter; in which vote of thanks I will include the distinguished home talent that has contributed to the enjoyment and enlightenment of the occasion, especially the representative from our own worthy Columbia University—Dr. Pupin.

THE CHAIRMAN:—All those in favor of this very graceful motion will please signify it by saying aye.

[The motion was carried.]

1. J. Clerk Maxwell, article on "The Atom," *Ency. Brit.*

[COMMUNICATED AFTER ADJOURNMENT BY CHARLES T. RITTENHOUSE.]

I am in thorough accord with the sentiments expressed by Dr. Kennelly in the course of his remarks on Wednesday evening last, to the effect that every endeavor should be made to co-ordinate the results of experiments with Röntgen rays with the generally accepted theories of light, rather than to propose new theories, concocted without mature deliberation, likely to be upset at any moment by the announcement of some new phenomenon. It seems to me that it would be preferable to withhold opinions regarding the exact nature of Röntgen rays until a sufficient number of facts have been collected to form a basis for the advancement of a new theory.

Allusion was made during the course of the discussion to the reflection of Röntgen rays, and apparently little credence is placed in the results of investigations thus far pursued in this direction. The presentation of a conclusive proof of regular reflection, although it would add little more to one theory than to another, still would be a most valuable contribution to our knowledge. Several investigators, both here and abroad announced in the early spring and since, that they had found Röntgen rays to be regularly reflected, but as many more contradicted these assertions so that the results as a whole have been discredited.

Had an opportunity offered itself it was my intention to bring before the members present, the results of some experiments, which although I do not consider absolutely conclusive, still indicate that regular reflection of Röntgen rays exists. These results I wish now to bring to the attention of the INSTITUTE, believing that they deserve more careful consideration than they have apparently received thus far. The experiments herein outlined were performed by Prof. O. N. Rood, of Columbia University, and described by him in *Science* of March 27th last, also before the Washington meeting of the National Academy of Sciences, and with important additions more recently in the *American Journal of Science*. In the second set of experiments, plane metallic surfaces of both platinum and speculum metal were employed as reflectors, and the effect produced on a sensitive photographic plate by Röntgen rays compared with the effect obtained under the same conditions, but when using ordinary light. The method employed was as follows: Röntgen rays from a vacuum tube were allowed to fall upon a vertical plane metallic surface at an angle of approximately 45 degrees, thence reflected either regularly or by diffusion to a sensitive plate protected by a screen, found impervious to the direct action of sunlight after an exposure of eight hours, and also by a plate of aluminium. In front of the plate holder was placed a wire netting. Direct action between the tube and the sensitive plate was prevented by interposing a thick metallic screen. Owing to

the fact that the Röntgen rays emanated from the tube in a diverging beam, it follows that a large part of the reflecting surface should become a secondary source of radiation so that the entire surface of the sensitive plate should be affected. Thus the angle of incidence ranged from 30 to 60 degrees. As a consequence of the arrangement of the apparatus, the vertical wires of

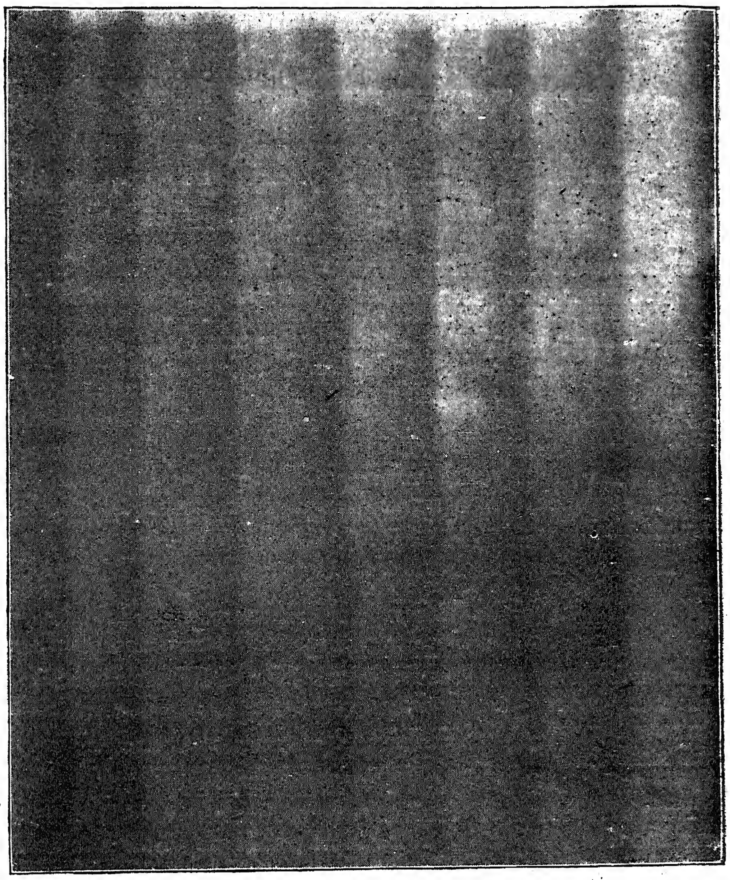


FIG. 5.

the netting produced a blurred image on the plate, due both to regularly reflected and diffused rays, while the horizontal wires produced a sharp image, due to the regularly reflected rays. Speculum metal although having a much smaller density than platinum, in these experiments gave the sharper horizontal lines. Professor Rood explained this fact as due to its smoother surface

and freedom from pimples. The prints thus obtained I have examined, but they are too faint to admit of reproduction.

The greenish light accompanying the Röntgen rays was now allowed to fall on a piece of white chalked paper that was substituted for the platinum or speculum mirror, its size and position corresponding with that of the mirrors. From the chalked

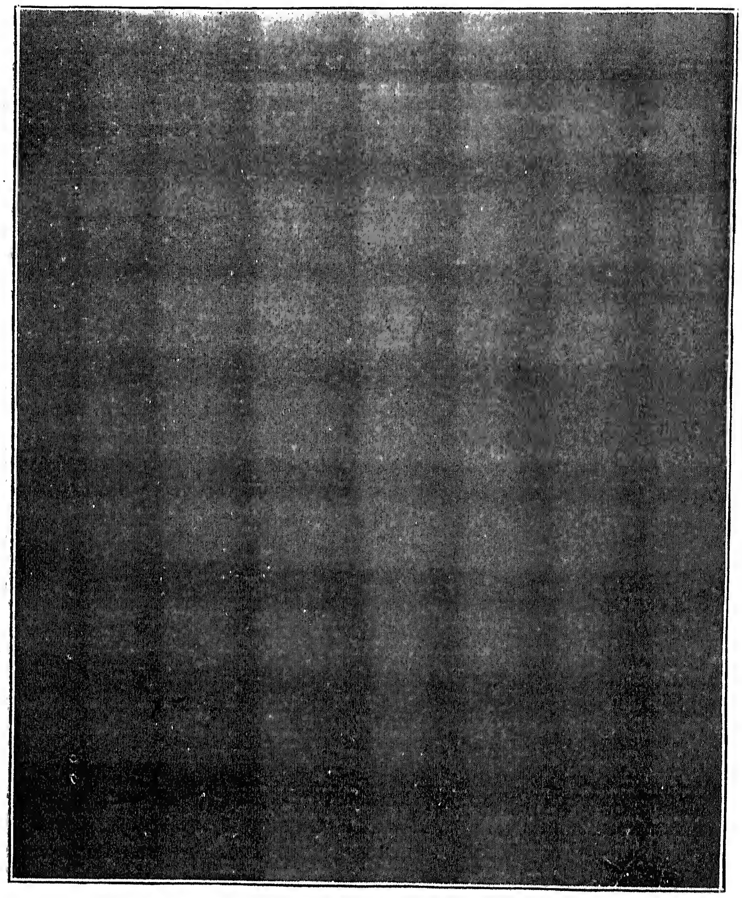


FIG. 6.

paper it reached the naked sensitive plate and there produced an image by strictly diffused light. This image was totally different in appearance from that obtained by the speculum mirror as it showed no trace of horizontal lines, they being replaced by broad streaks. It bore some resemblance to the Röntgen ray picture obtained from the platinum mirror, but still was different from

it. It is difficult to account for this result except on the supposition that a portion of the Röntgen rays had in the experiments with the mirrors undergone regular or speculum reflection.

In the third set of experiments a concave cylindrical mirror with a curvature not truly circular, was substituted for the plane mirrors which had been previously used, the object being to cause by specular reflection, a beam of light to produce blurred or doubled shadows on the vertical wires of the grating, and at the same time give sharp single shadows of the horizontal wires. The relation between the ordinary Crookes tube employed and the cylindrical mirror and plate holder was such as to bring about this condition when ordinary light from the tube was used. If similar results could be obtained when Röntgen rays were substituted for ordinary light, it was highly probable that regular reflection of these rays exists. Fig. 5 is the result of the experiment. The horizontal lines although sharp as compared with the vertical lines, are accompanied by fainter parallel lines due to the Röntgen rays which have suffered diffusion ; the vertical lines it will be noticed are broadened considerably. In sharp contrast with this figure, is Fig. 6 which was produced by the diffusion of ordinary light from a chalked surface under conditions exactly similar, with the exception that the sensitive plate was unprotected, to those present when Röntgen rays were used. Thus it will be seen that there are strong indications for believing that regular reflection of Röntgen rays exists.

In conclusion it may be said that when it is considered that Röntgen rays most likely consist of vibrations of such small wavelengths as to be comparable with the distances between the molecules of the most dense substances, it can scarcely be expected that regular reflection will take place to any large extent, but that dense substances having a most highly polished surface will still be as rough to these rays as a badly polished surface to ordinary light. In other words, regular reflection and diffusion are relative terms only, so that a given surface subjected to one mode of vibration behaves entirely different when receiving vibrations of a higher or lower order.

OBITUARY.

Henry Adams Craigin (Associate Member, June 6th, 1893, Member, May 15th, 1894,) was born at Boston, May 14th, 1867. He was connected with the Adams family of New England through Samuel Adams of Revolutionary fame, of whom he was a direct descendant.

He was graduated from the English High School of Boston, and entered the Massachusetts Institute of Technology in 1885, and was graduated in 1889, receiving the degree of Bachelor of Science in the course of mechanical engineering. During the years 1888 and 1889, in addition to his regular studies, he took the principal part of the course in electrical engineering, and his summer vacations were spent in connection with the United States Geological Survey, for which he was during the summer of 1889, assistant geologist.

In the fall of 1889 he entered the shops of the Union Pacific Railway Company, at Omaha, Neb., and, working through various branches, became Section Superintendent, and afterward Chief Inspector of Railway Supplies, and Purchasing Agent. During his service with the Union Pacific Railway Company he travelled extensively over the system of that, and other western railways for the purpose of collecting data. He resigned his position with the Union Pacific Railway in 1891, and spent the summer of 1891 in travelling over the Western States and Northern Mexico, studying railroad and other engineering problems, and inspecting a railroad in southern Texas for certain capitalists. During the years 1891 and 1892, he was Superintendent and General Manager of the Motor and Railway Company at Tacoma, Wash. He was also in the employ of the Portland Bridge and Iron Works, of Portland, Ore., for several months.

Electrical engineering had always been an attractive field to Mr. Craigin, and in the year 1892 he went to Pittsburgh in the employ of the Westinghouse Electric and Manufacturing Company. The success of his labors with the Westinghouse company amply proved his abilities in this direction.

In the year 1893 he was attached to the New York office of the Westinghouse company in the capacity of an electrical engineer, and was engaged in the selling and engineering branches of the business of that company at the time of his death, which occurred in the City of Mexico on November 27th, 1896. He had gone to Mexico partly for rest and partly in the interests of the company, but while there contracted a severe cold which resulted in pneumonia, the cause of his death.

Mr. Craigin was deeply interested in his professional work at all times, and his energy and perseverance had won for him the highest esteem of all his associates. His ability in electrical engineering was well recognized. He had devoted much time the last few months to the study of engineering problems involved in the proposed electric railway between Washington and Baltimore.

Outside of business, he was very much interested in music, and was well versed in English and French literature. He was fond of out-door athletics, and at the time of his death was a member of the Staten Island Cricket and Base Ball Club, the Boston Athletic Association, and was also a member of other social and literary organizations.

He was a man of the highest integrity and nobility of character, and by his death this INSTITUTE and the electrical profession are deprived of an able, energetic and valued member.

HARVEY L. LUFKIN (Associate Member June 17th, 1890) was born in Cleveland, Ohio, in 1857, and died in New York City December 21st, 1896, after an illness of but four days.

Mr. Lufkin had been engaged in electrical work for fifteen years. In 1886 he became associated with Messrs. Curtis, Crocker and Wheeler in establishing the C. & C. Electric Motor Company and played an important part in introducing and developing electric power. He was one of the first to operate electric motors on arc and incandescent lighting circuits, in spite of the strong opposition of the central station officers, who

looked upon this new application as a dangerous experiment. Fortunately he lived long enough to see electric power distribution become a very large and welcome part of the business of central stations. He was a pioneer in applying electric motors to the operation of factories and mills, and also had the satisfaction of witnessing the success and general acceptation of this improved method. Mr. Lufkin possessed the rare combination of technical knowledge with great business ability, enabling him to accomplish results which would be impossible for a man having either one of these qualities alone. This advantage, together with his excellent judgment, prevented him from making the mistakes which are so common in the development of a new art, and gave him a power to foresee the probable directions of successful progress, two examples of this prescience having already been noted. Although enthusiastic as to the value and possibilities of electrical applications, his good sense told him just where to stop, and it often happened that he would advise against the use of electrical apparatus when he did not think that it had decided advantages over other means, even though the electrical method might be perfectly feasible.

Mr. Lufkin was a very active member of the National Electric Light Association, and was a leading spirit in organizing and making a notable success of the Electrical Exhibition held in New York City during the spring of 1896.

Possessing an unusually bright and agreeable personality, he was universally popular among electrical men, and was sincerely liked by his friends and business associates.

His sudden death in the prime of his activity and usefulness is a great loss to the electrical industry, which always requires the services of men of exactly his stamp. F. B. C.

American Institute of Electrical Engineers.

CATALOGUE OF MEMBERS.

March 1st, 1897.

HONORARY MEMBERS.

Name.	Address.	Date of Membership.
Kelvin, *Lord*,	*LL.D.*, *F.R.S.S.L.* and *E.* The University, Glasgow, Scotland,	H.M. May 17, 1892
Preece, Wm. H. *F.R.S.*	Electrician, General Post Office, London, Eng. Residence, Gothic Lodge, Wimbledon.	H.M. Oct. 21, 1884

Total, 2.

MEMBERS.

Abbott, Arthur V.	Chief Engineer, Chicago Telephone Co., 203 Washington St., Chicago, Ill.	A Oct. 21, 1890 M Jan. 16, 1895
Acheson, Edw. G.	President, The Carborundum Co., Niagara Falls, N. Y.	A Jan. 3, 1888 M May 1, 1888
Adams, Alton D.	Electrical Engineer, 620 Atlantic Ave., Boston, Mass.	A April 18, 1893 M Jan. 17, 1894
Ahearn, T.	Ahearn & Soper, Electrical Supplies, Ottawa, Ont.	A July 12, 1887 M Sept. 6, 1887
Albright, H. Fleetwood	Electrical Engineer, Western Electric Co., New York; residence, 60 Sayre St., Elizabeth, N. J.	A Sept. 27, 1892 M June 20, 1894
Almon, G. H.	Electrical Engineer and Contractor, 136 Liberty St., New York City, and 620 Atlantic Ave., Boston, Mass.	A Sept. 20, 1893 M Mar. 21, 1894
Andrews, Wm. S	Manager Central Station Sales, General Electric Co., Schenectady, N. Y.	A Mar. 5, 1889 M April 22, 1896
Anthony, Prof. W. A.	(*Past President.*) Consulting Electrician, Temple Court, New York, N. Y.; residence, 675 High St., Newark, N. J.	A Dec. 9, 1884 M Jan. 6, 1885

Name.	Address.	Date of Membership.
ARNOLD, BION J.	(*Manager.*) Consulting Electrical Engineer, 1541 Marquette Bldg. and 4128 Prairie Ave., Chicago, Ill.	A Oct. 25, 1892 M Nov. 15, 1893
AYER, JAMES I.	General Manager American Electric Heating Corporation, 611 Sears Building, Boston, Mass.	A May 19, 1891 M April 19, 1892
AYRES, BROWN	Professor of Physics and Electrical Engineering, Tulane University, New Orleans, La.	A Dec. 16, 1891 M Mar. 15, 1892
BADT, LIEUT. FRANCIS B.	Electrical Engineer, Siemens & Halske Electric Co. of America, 1215 Monadnock Block and 6506 Lafayette Ave., (Englewood), Chicago, Ill.	A April 19, 1892 M Mar. 25, 1896
BAILLARD, E. V.	Manufacturer of Electrical Instruments and Fine Machinery, 106 Liberty St., New York City.	A Dec. 3, 1889 M Jan. 16, 1895
BALDWIN, BERT L.	Mechanical and Electrical Engineer, The Cincinnati Street R'way Co., 72 Perin Bldg., Cincinnati, O.	A April 22, 1896 M Nov. 18, 1896
BATCHELOR, CHAS.	Electrical Engineer, 33 West 25th St., New York City.	A June 8, 1887 M July 12, 1887
BATES, J. H.	Assistant Engineer and Draughtsman, with C. J. Bates & Co., 126 Liberty St., New York City, and 321 Hudson St., Hoboken, N. J.	A Sept. 6, 1887 M Oct. 1, 1889
BAYLIS, ROBERT NELSON	Electrical Engineer, The Walker Manufacturing Co., Cleveland, O.	A Oct. 1, 1889 M May 17, 1892
BEDELL, DR. FREDERICK,	Assistant Professor in Physics, Cornell University, Ithaca, N. Y.	A April 21, 1891 M May 19, 1896
BELL, PROF. A. GRAHAM	(*Past President.*) 1331 Conn. Ave., Washington, D. C., and Baddeck, N. S.	A April 15, 1884 M Oct. 21, 1884
BELL, DR. LOUIS	Electrical Engineer, Boston, Mass.	A May 20, 1890 M June 18, 1890
BENJAMIN, PARK	Electrical Expert and Engineer, 203 Broadway, N. Y. City.	A Dec. 16, 1891 M Feb. 16, 1892
BERNARD, EDGAR G.	Electrical Engineer, President, E. G. Bernard & Co., 43 4th St., Troy, N Y.	A. Jan. 5, 1886 M July 12, 1887
BERTHOLD, VICTOR M.	Patent Department, American Bell Telephone Co., 125 Milk St., Boston; residence, 16 Upton St., Cambridgeport, Mass.	A May 17, 1892 M May 21, 1895
BILLBERG, C. O. C.	Electrical Engineer, Carbondale, Pa.	A Mar. 21, 1894 M Feb. 27, 1895
BINNEY, HAROLD	Patent Solicitor and Expert, Potter Building, 38 Park Row, New York City.	A Sept. 16, 1890 M Dec. 16, 1890
BIRDSALL, E. T. *M. E.*	Consulting Electrical Engineer, 18 Broadway; Residence, 56 West 38th St., New York City.	A June 8, 1887 M Nov. 1, 1887

Name.	Address.	Date of Membership.
BISHOP, JAMES DRAPER	Electrical Engineer of the Safety Insulated Wire and Cable Co., 234 W. 29th St., New York City.	A Dec. 16, 1891 M Oct. 25, 1892
BLADES, HARRY H.	Electrical Engineer, 419 Cass Ave., Detroit, Mich.	A April 19, 1892 M May 21, 1895
BLAKE, FRANCIS	Auburndale, Mass.	A Sept. 3, 1889 M Oct. 1, 1889
BLODGETT, GEO. W.	Electrical Engineer, B. & A. R. R. and Consulting Electrician, Boston, Mass.	A July 12, 1887 M Sept. 6, 1887
BLOOD, JOHN BALCH	Union Elektricitäts Ges., 32 Holmannstrasse, Berlin, Germany.	A June 20, 1894 M Dec. 18, 1895
BOILEAU, WILLARD E.	Superintendent and Electrician, Brush Electric Light & Power Co., Columbus, Ga.	A Sept. 19, 1894 M Mar. 25, 1896
BOSCH, ADAM	Sup't Fire Alarm Telegraph, Newark, N. J.	A April 15, 1884 M Jan. 6, 1885
BOSSON, FREDERICK N.	Consulting Mining and Electrical Engineer; Electrician, Calumet and Hecla Mining Co., Calumet, Mich.	A May 17, 1892 M Feb. 21, 1893
BOTTOMLEY, HARRY	Electrical Engineer, Supt., Marlboro Electric Co., Marlboro, Mass.	A April 2, 1889 M Jan. 22, 1896
BOURNE, FRANK	Electrical Engineer, 39 Cortlandt St., New York City.	A April 21, 1891 M Nov. 15, 1892
BOWMAN, FRED. A.	Supt., New Glasgow Electric Co., New Glasgow, Nova Scotia.	A May 19, 1891 M Nov. 21, 1894
BOYER, ELMER E.	Foreman, Testing Department, Lynn Works, General Electric Co., Lynn, Mass.	A Sept. 25, 1895 M Mar. 25, 1896
BOYNTON, EDWARD C.	Electrical Dep't, N. Y., N. H. & H. R. R., New Haven, Ct.	A Aug. 6, 1889 M Nov. 24, 1891
BRADLEY, CHAS. S.	(*Manager.*) Electrical Engineer, 44 Broad Street, New York City.	A May 24, 1887 M Dec. 6, 1887
BRENNER, WILLIAM H.	Electric Storage Battery Co., 66 Broadway, New York City.	A Sept. 20, 1893 M Mar. 21, 1894
BRINCKERHOFF, HENRY MORTON	Electrical Engineer, Metropolitan West Side Elevated R. R.; 258 Franklin St., Chicago, Ill.	A Sept. 23, 1896 M Dec. 16, 1896
BROADNAX, FRANCIS	Electrical Engineer, Blake and Williams, 362 West Broadway, New York City; residence, 28 Walnut St., Montclair, N. J.	A Jan. 17, 1894 M Jan. 16, 1895
BROOKS, MORGAN	President and Manager, The Electrical Engineering Co., 249 Second Ave., South; residence, 2950 Park Ave., Minneapolis, Minn.	A May 20, 1890 M June 17, 1890
BROWN, ALFRED S.	Electrical Engineer, Western Union Telegraph Co., 195 Broadway, P. O. Box 856, New York City.	A Mar. 18, 1890 M Feb. 21, 1893

Name.	Address.	Date of Membership.
BROWN, EDWARD D.	General Inspector, American Telephone and Telegraph Co., 18 Cortlandt St, New York City; residence, 75 Hicks St., Brooklyn, N. Y.	A Sept. 19, 1894 M Nov. 20, 1895
BROWN, J. STANFORD. *E.E.*, [Life Member.]	Consulting Electrical Engineer; Carpenter Steel Co., 1 Broadway and 100 Broadway, New York City; residence, Park Hill, Yonkers, N. Y.	A Sept. 6, 1887 M Nov. 1, 1887
BRUSH, CHAS. F.	Electrical Engineer, 453 The Arcade, Cleveland, O.	A April 15, 1884 M Oct. 21, 1884
BURLEIGH, CHAS. B.	Supt. Isolated Dept. General Electric Co., 620 Atlantic Ave., Boston, Mass.	A April 21, 1891 M Feb. 16, 1892
BYLLESBY, HENRY M.	Northwest General Electric Co., 403 Sibley St., St. Paul, Minn.	A Sept. 7, 1888 M Oct. 2, 1888
CAHOON, JAS. B.	Electrical Engineer; General Manager, The Elmira Municipal Improvement Co., 313 Columbia St., Elmira, N. Y.	A June 17, 1890 M May 19, 1891
CALLENDER, ROMAINE	Electrician, Ryton-on-Tyne, Eng.	A Sept. 27, 1892 M May 21, 1895
CARHART, HENRY S.	Prof. of Physics, University of Michigan, Ann Arbor, Mich.	A Sept. 25, 1895 M April 22, 1896
CARROLL, LEIGH	Algiers Ice and Electric Co., 423 Baronne St., New Orleans, La.	A Oct. 1, 1889 M Nov. 12, 1889
CARUS-WILSON, CHARLES A.	Professor of Electrical Engineering, McGill University, Montreal, P. Q.	A April 18, 1894 M April 17, 1895
CHAMBERLAIN, J. C.	Electrical Engineer, Morris Heights; residence, 1 West 81st St., New York City.	A Dec. 6, 1887 M Jan. 3, 1888
CHANDLER, PROFESSOR CHARLES F.	Columbia University 41 East 49th St., New York City.	A Jan. 20, 1891 M June 7, 1892
CHASE, HARVEY STUART	Mechanical and Electrical Engineer, 8 Congress St., Boston, Mass.	A Sept. 19, 1894 M Jan. 22, 1896
CHENEY, W. C.	Superintendent, Consolidated Railway Co., Victoria, B. C.	A Sept. 22, 1891 M Nov. 21, 1894
CHILDS, ARTHUR EDWARDS, *B. Sc. M.E.E.E.*	Manager New England Office, The Electric Storage Battery Co., 92 State Street, Boston, Mass.	A June 20, 1894 M April 17, 1895
CHURCHILL, ARTHUR	British Thomson-Houston Co., 83 Cannon Street, London, Eng.	A April 15, 1890 M Jan. 17, 1893
CLARK, ERNEST P.	Electrical Engineer, Clark Electric Co., 478 Pearl St., New York City.	A Jan. 8, 1887 M Nov. 1, 1887
CLARKE, CHAS. L.	Electrical Engineer and Patent Expert, 31 Nassau St., New York City.	A April 15, 1884 M Jan. 6, 1885
COLBY, EDWARD A.	Consulting Engineer, Lock Box 313, Newark, N. J.	A April 2, 1889 M May 7, 1889

Name.	Address.	Date of Membership.
COLVIN, FRANK R.	203 Broadway, New York City.	A April 18, 1894 M May 21, 1895
COMSTOCK, LOUIS K.	Electrical Engineer, 1108-9 Fort Dearborn Building, Chicago, Ill.	A Dec. 20, 1893 M Nov. 20, 1895
CONDICT, G. HERBERT	Electrical Engineer, 5328 Green St., Germantown, Pa.	A July 12. 1887 M Sept. 6, 1887
COSTER, MAURICE	Engineer, Westinghouse Elec. and Mfg. Co., N. Y. Life Bldg,, Chicago, Ill.	A Sept. 25, 1895 M Mar. 25, 1896
CORNELL, CHAS. L.	Electrical Engineer, Hamilton, O.	A Feb. 7 1890 M June 27, 1895
COTHREN, WM. H.	51 W. 37th St., New York City.	A Aug. 6, 1889 M Oct. 1, 1889
COWLES, ALFRED H.	Technical Adviser to the Cowles Smelting and Aluminum Co., 656 Prospect St., Cleveland, O.	A Mar. 5, 1886 M May 7, 1889
CRAIG, J. HALLY	New England Electrical Supply Co., 49 Federal St., Boston, Mass.	A May 16, 1893 M Feb. 27, 1895
CRANDALL, JOSEPH EDWIN	Electrician, C. & P. Telephone Co., 619 Fourteenth St., N. W. Washington, D. C.	A April 18, 1892 M April 18, 1894
CROCKER, FRANCIS BACON [Life Member.]	Professor of Electrical Engineering, Columbia University, New York.	A May 24, 1887 M April 2, 1889
CROSBY, JAMES WELLINGTON	Electrical Engineer, Room 102, 15 Federal St., Boston; residence, Wellington, Mass.	A Feb. 21, 1894 M Feb. 27, 1895
CROSS, CHARLES R.	Thayer Professor of Physics, and Director of the Rogers Laboratory, Mass. Institute of Technology, Boston, Mass.	A April 15, 1884 M Oct. 21, 1884
CUSHING, HARRY COOKE, JR.	Electrical Consulting and Constructing Engineer, 39 Cortlandt St., New York City.	A Sept. 19, 1894 M Nov. 18, 1896
CUTTER, GEORGE	Dealer in Electrical Supplies, 851 The Rookery, Chicago, Ills.	A June 17, 1890 M May 19, 1891
CUTTRISS, CHAS.	Electrician, The Commercial Cable Co., 1 Broad St., New York.	A Nov. 1, 1887 M Dec. 6, 1887
DAFT, LEO	Consulting Electrical Engineer and Contractor, 633 W. 21st Street, Los Angeles, Cal.	A Dec. 9, 1884 M Jan. 6, 1885
DANIELL, FRANCIS G.	Electrical Engineer, Fairhaven and Westville R. R. Co., P. O. Box 394, New Haven, Conn.	A Nov. 12, 1889 M June 20, 1894
DARLINGTON, FREDERIC W.	Consulting Electrical and Mechanical Engineer, 907 Drexel Building, Philadelphia, Pa.	A Sept. 19, 1894 M Nov 25, 1895
DAVIES, JOHN F.	Professor of Physics, University of Wisconsin, 523 North Carroll St., Madison, Wis.	A Jan. 7, 1890 M Mar. 18, 1890

Name.	Address.	Date of Membership.
DAVIS, CHARLES H., *C. E.*,	Consulting and Constructing Engineer, 99 Cedar St., 576 Lexington Ave., New York City, and 308 Walnut St., Philadelphia, Pa.	A Mar. 18, 1890 M June 17, 1890
DAVIS, MINOR M.	Ass't Electrician, Postal Telegraph-Cable Co., 253 Broadway, New York City.	A April 6, 1886 M May 16, 1893
DAWSON, PHILIP	Associate and Chief Engineer with R. W. Blackwell, 39 Victoria St., Westminster, London, Eng.	A Sept. 25, 1895 M Feb. 17, 1897
DELAFIELD, A. FLOYD, *Ph. D.*	Electrical Engineer, Noroton, Conn.	A May 7, 1889 M Oct. 1, 1889
DELANY, PATRICK BERNARD	Inventor, South Orange N. J.	A April 19, 1884 M Nov. 24, 1891
DICKENSON, SAMUEL S.	Sup't, Commercial Cable Co., Hazel-Hill, Guysborough Co., N. S.	A Mar. 6, 1888 M Oct. 1 1889
DIEHL, PHILIP	Inventor, Singer Sewing Machine Co., 508 Morris Ave., Elizabeth, N. J.	A April 15, 1884 M Dec. 9, 1884
D'INFREVILLE, GEORGES	Electrical Engineer and Expert, 10 Desbrosses St., New York City.	A Nov. 1, 1887 M Dec. 6, 1887
DION, ALFRED A.	General Supt., The Ottawa Electric Co, 72 Sparks St., Ottawa, Ont.	A Jan. 7, 1890 M Nov. 15, 1893
DOANE, S. EVERETT	101 State St., Schenectady, N. Y.	A Aug. 6, 1889 M June 27, 1895
DODGE, PROF. OMENZO G.,	U. S. Navy, Navy Dep't, Washington, D. C.	A Sept. 20 1893 M April 17, 1895
DOIJER, H.	Consulting Electrical Engineer, 8 Choorstraat, Delft, Holland.	A Jan. 7, 1890 M Mar 18, 1890
DOMMERQUE, FRANZ J.	Chief Draughtsman, Chicago Telephone Co.; residence, 496 N. Robey St., Chicago, Ill.	A Oct. 17, 1894 M Mar 25, 1896
DONNER, WILLIAM H.	Electrical Eng'g Dept. International Correspondence School, Scranton, Pa.	A Nov. 18, 1890 M Dec. 16, 1890
DOW, ALEX	Manager, Edison Illuminating Co., Detroit, Mich.	A Sept. 20, 1893 M Dec. 18, 1895
DUDLEY, CHARLES B.	Chemist and Scientific Expert, Penn. R. R. Co., 1219 Twelfth Ave., Altoona, Pa.	A Oct. 1, 1889 M Nov. 12, 1889
DUNBAR, F. W.	417 West 23d St., New York City.	A Dec. 21, 1892 M May 16, 1893
DUNCAN, DR. LOUIS	(*President*) Johns Hopkins University, residence, 139 E. North Ave., Baltimore, Md.	A July 12, 1887 M Sept. 6, 1887
DUNN, GANO SILLICK	Chief Engineer, Crocker-Wheeler Electric Co., Ampere, E. Orange, N. J.; residence, 223 Central Park, West, New York City.	A April 21, 1891 M June 20, 1894
DUNSTON, ROBT. EDWARD	The Cortland and Homer Traction Co., Cortland, N. Y.	A Oct 27, 1891 M Feb. 16, 1892
DYER, R. N.	Patent Attorney, 36 Wall St., New York City.	A July 12, 1887 M Sept. 6, 1887

Name.	Address.	Date of Membership.
EDISON, THOMAS A.	Mechanician and Inventor, Orange, N. J.	A April 15, 1884 M Oct. 21, 1884
EDGAR, C. L.	General Manager and Chief Engineer, Edison Elec. Ill'm'g Co., 3 Head Place, Boston, Mass.	A Jan. 22, 1896 M May 19, 1896
EGGER, ERNST	Electrical Engineer care of B. Egger & Co, X., Simmeringstr, 187, Vienna, Austria.	A Feb 21, 1893 M Mar. 21, 1894
EMERY, CHARLES EDWARD	Consulting Engineer, 915 Bennett Building, cor. Fulton and Nassau Sts., New York City.	A June 26, 1891 M April 19, 1892
EMMET, W. L. R.	Electrical Engineer, General Electric Co., Schenectady, N. Y.	A June 6, 1893 M Jan. 17, 1894
EVEREST, AUGUSTINE R.	Electrical Engineer, General Electric Co., Lynn, Mass.	A May 19, 1891 M Dec. 20, 1893
FARNHAM, ISAIAH H.	Electrical Engineer, N. E. Telephone & Telegraph Co., 125 Milk St., Boston, Mass.	A June 8, 1887 M July 12, 1887
FESSENDEN, REGINALD A.	Professor of Electrical Engineering, Western University of Pennsylvania, Allegheny, Pa.	A Oct. 21, 1890 M Dec. 16, 1890
FIELD, C. J., *M. E.*	Consulting and Constructing Engineer, 39 Cortlandt Street, New York City.	A June 8, 1887 M Nov. 1, 1887
FIELD, HENRY GEORGE	Consulting Electrical Engineer, Field & Hinchman, 1203 Majestic Building, Detroit, Mich.	A April 22, 1896 M Dec. 16, 1896
FIELD, STEPHEN D.	Electrical Engineer, Stockbridge, Mass.	A April 15, 1884 M Oct. 21, 1884
FISH, WALTER CLARK	Manager Lynn Works, General Electric Co., Lynn, Mass.	A June 26, 1891 M Feb. 26, 1896
FITZMAURICE, JAMES S.	Chief Engineer, The Electric Light Branch, 210 George St, Sydney, N. S. W.	A Sept. 20, 1893 M Mar. 21, 1894
FLACK, J. DAY	Burhorn & Granger, Contracting Mechanical Eng'rs, 136 Liberty St., New York City; residence, 80 Carlton St., East Orange, N. J.	A Dec. 6, 1887 M May 21, 1895
FORTENBAUGH, S. B.	Asst. Prof. of Electrical Engineering, University of Wisconsin, Madison, Wis.	A April 17, 1895 M Dec. 16 1896
FOSTER, HORATIO A.	Electrical Engineer, Room 656, Ellicott Square, Buffalo.	A June 8, 1887 M Sept. 6, 1887
FOSTER, SAMUEL L.	Electrical Engineer, Market Street Railway Co. 19 Hobart Bldg.; residence, 3687 24th St., San Francisco, Cal.	A Feb. 26, 1896 M Nov. 18, 1896
FREEMAN, DR. FRANK L.	Attorney-at-Law. Solicitor of Patents, Electrical Expert, 931 F St., Washington, D. C.	A May 7, 1889 M Sept. 3, 1889
FREEDMAN, WILLIAM H.	Tutor in Electrical Engineering, School of Mines, Columbia University; residence, 157 W. 119th St., New York City.	A Mar. 18, 1890 M Dec. 18, 1895

Name.	Address.	Date of Membership.
GALE, HORACE B.	Electrical and Mechanical Engineer, American Electric Heating Corporation, 610 Sears Building, Boston; residence, Natick, Mass.	A Nov. 15, 1892 M May 16, 1893
GARDANIER, GEORGE W.	Assis't Electrical Engineer, Western Union Telegraph Co., 195 Broadway, New York City.	A April 18, 1893 M Jan. 22, 1896
GARRATT, ALLAN V.	Chief Engineer, Lombard Waterwheel Governor Co., 61 Hampshire St., Boston, Mass.	A April 2, 1889 M May 7, 1889
GERRY, M. H., JR.	Supt. of Motive Power, The Metropolitan West Side Elevated Railroad Co, 146 Throop St., Chicago, Ill.	A April 18, 1893 M Oct. 21, 1896
GEYER, DR. WM. E.	Stevens Institute of Technology, Hoboken, N. J.	A June 5, 1888 M Sept. 7, 1888
GHARKY, WILLIAM DAVID	Sup't Underground Cable Construction and Maintenance, Philadelphia Traction Co.; 820 Dauphin St., Philadelphia, Pa.	A May 21, 1895 M Feb. 26, 1896
GIBBS, LUCIUS T.	Manager and Chief Engineer, Gibbs Electric Co., Milwaukee, Wis.	A Mar. 25, 1896 M Feb. 17, 1897
GIFFORD, CLARENCE E.	Electrical Engineer, Supt. Jamestown Electric Light and Power Co., Jamestown, N. Y.	A May 16, 1893 M Feb. 21, 1894
GRAY, DR. ELISHA	Electrician and Inventor, Highland Park, Ill.	A Feb. 16, 1892 M May 17, 1892
GREENE, S. DANA	Assistant General Manager, General Electric Co., Schenectady, N. Y.	A Sept. 20, 1893 M April 18, 1894
GRISCOM, WM. W., *M.A.*	Electrical Engineer, 224 Chestnut St., Philadelphia; residence, Haverford, Pa.	A June 5, 1888 M Mar. 18, 1890
GUTMANN, LUDWIG	Electrical Engineer, 815 North Jefferson Ave., Peoria, Ill.	A Sept. 14, 1888 M Mar. 21, 1893
HADAWAY, W. S., Jr.	Electric Heating Engineer, 107 Liberty St., New York City.	A Nov. 21, 1894 M Oct. 21, 1896
HALL, CLAYTON C.	Civil Engineer, 810 Park Ave., Baltimore, Md.	A April 15, 1884 M Oct. 21, 1884
HALL, JOHN L.	The Electrical Maintenance Co., 44 N. 4th St., Philadelphia, Pa.; residence, 715 W. 10th St., Wilmington, Del.	A Sept. 22, 1891 M Dec. 20, 1893
HAMBLET, JAMES	Manager Time Service, W. U Tel. Co., 195 Broadway, P. O. Box 856, NewYork City; residence, 20 Sidney Place, Brooklyn, N. Y.	A Nov. 1, 1887 M Dec. 6, 1887
HAMILTON, GEO. A.	(*Treasurer.*) Electrician, Western Electric Co., 22 Thames, cor. Greenwich St., New York; residence, 532 Morris Ave, Elizabeth, N. J.	A April 15, 1884 M Oct. 21, 1884
HAMMER, WILLIAM J.	Consulting and Supervising Electrical Engineer, 1305 Havemeyer Bldg, 26 Cortlandt St., New York City; residence, Elmora, N. J.	A June 8, 1887 M July 12, 1887

Name.	Address.	Date of Membership.
HARRINGTON, WALTER E.	Electric Railway Engineer, 307 Market St., Camden, N. J.	A Mar. 17, 1891 M May 19, 1896
HARRISON, RUSSELL B.	Pres. and Electrical Engnieer, Terre Haute Electric Railway Co., Terre Haute, Ind.	A Sept. 25, 1895 M April 22, 1896
HARTWELL, ARTHUR	Electrical Engineer, Westinghouse Electric and Mfg Co.; residence, 6804 McPherson St., Pittsburg, Pa.	A May 15, 1894 M Nov. 20, 1895
HASKINS, CARYL D.	Electrical Engineer, General Electric Co., 620 Atlantic Ave., Boston, Mass.	A Mar, 18, 1890 M June 20, 1894
HASKINS, CHARLES H.	Electrician, 70 Linwood Avenue, Buffalo, N. Y.	A April 15, 1884 M Oct. 21, 1884
HASKINS, CLARK CARYL	City Electric Light Inspector, 582 West Congress St., Chicago, Ill.	A Sept. 20, 1893 M Mar. 21, 1894
HASSON, W. F. C.	(*Vice-President.*) Firm of Hasson & Hunt, Consulting and Supervising Mechanical and Electrical Engineers, 310 Pine St., Telephone 5650, San Francisco, Cal.	A Mar. 18, 1890 M May 15, 1894
HAYES, HAMMOND V.	Electrician, American Bell Telephone Co., 42 Farnsworth St., So. Boston, Mass.	A Nov. 12, 1889 M Mar. 18, 1890
HAYES, HARRY E.	Asst. Electrician, American Telegraph and Telephone Co., 153 Cedar St., New York City.	A April 18, 1893 M Dec. 20, 1893
HAYNES, F. T. J.	Divisional Telegraph Engineer, Great Western Railway; residence, Belmont Villa, Cheddon Road, Taunton, Eng.	A Dec. 6, 1886 M Jan. 3, 1887
HEATH, HARRY E.	Assistant Electrical Engineer, Eddy Electric Mfg. Co., Box 189, Windsor, Conn.	A Mar. 21, 1893 M Mar. 25, 1896
HEINRICH, RICHARD O.	Electrical Engineer, The European Weston Electrical Instrument Co., Köpnicker Strasse 154, Berlin, S. O. Germany.	A Oct. 1, 1889 M Oct. 25, 1892
HENSHAW, FREDERICK V.	Downes & Henshaw, Consulting Electrical and Mechanical Engineers, 95 Pine Street, Providence, R. I.	A Feb. 5, 1889 M Nov. 20, 1895
HERDMAN, FRANK E.	Mechanical and Electrical Engineer, Crane Elevator Co., Winnetka, Ill.	A Dec. 18, 1895 M Oct. 21, 1896
HERING, CARL [Life Member.]	(*Manager*) Consulting Electrical Engineer, 929 Chestnut St.; residence 124 E. Mt. Pleasant Ave., Philadelphia, Pa.	A Jan. 3, 1888 M June 5, 1888
HERING, HERMANN S.	Associate in Electrical Engineering, Johns Hopkins University, residence, 1809 Park Ave., Baltimore, Md.	A April 21, 1891 M April 18, 1893
HERRICK, CHARLES H.	Consulting and Constructing Electrical Engineer, 133 Oliver St., Boston; residence, 22 Herrick St., Winchester, Mass.	A April 21, 1891 M Jan. 17, 1893

Name.	Address.	Date of Membership.
HERZOG, F. BENEDICT, *Ph. D.*	President, Herzog Teleseme Co., 55 Broadway, New York City	A May 24, 1887 M July 12 1887
HEWITT, CHARLES	Electrical Engineer, Union Traction Co., 820 Dauphin Street, Philadelphia, Pa.	A Sept. 16, 1890 M May 17, 1892
HIBBARD, ANGUS S.	(*Vice-President.*) General Manager, Chicago Telephone Co., 203 Washington St., Chicago, Ill.	A Nov. 24. 1891 M Feb. 16, 1892
HIGGINS, EDWARD E.	Editor, *Street Railway Journal*, 26 Cortlandt St., New York City.	A June 8, 1887 M July 12, 1887
HIX, E. RANDOLPH	Hix, Hamilton & Co., Electrical Engineers and Contractors, 41 Wall St., New York City.	A Feb. 21, 1894 M Feb. 27, 1895
HOLMES, FRANKLIN S.	Electrical Engineer, 108 Fulton St., New York City; residence 445a Macon St., Brooklyn, N. Y.	A April 21, 1891 M June 20, 1894
HOUSTON, EDWIN J., *Ph.D.* [Life Member.]	(*Past President.*) Prof of Physics, Franklin Inst., Firm of Houston & Kennelly, 1105 Betz Bldg.; residence 1809 Spring Garden St., Philadelphia, Pa.	A April 15, 1884 M Oct. 21, 1884
HOWELL, JOHN W.	Electrician, 20 Chestnut St., Newark, N. J.	A July 12, 1887 M June 5, 1888
HOWELL, WILSON S.	General Electric Lamp Works, Harrison, N.J.; residence, 19 Webster Place, Orange, N. J.	A Sept. 3, 1889 M Mar. 18, 1890
HUNTER, RUDOLPH M.	Expert and Counsellor in Patent Causes, 926 Walnut St., Philadelphia, Pa.	A July 13, 1886 M May 17, 1887
HUNTING, FRED S.	Chief Engineer, Fort Wayne Electric Co., 330 West Washington St., Fort Wayne, Ind.	A Nov. 15, 1892 M May 16, 1893
HUTCHINSON, DR. CARY T.	(*Manager.*) Electrical Engineer, 253 Broadway, New York City.	A Feb. 7, 1890 M Dec. 16, 1890
HYDE, JEROME W.	Ass't Treasurer, The Springfield Steam Power Co., Wason Bldg. Springfield, Mass.	A June 8, 1887 M Nov. 1, 1887
INRIG, ALEC GAVAN	Globe Electrical Co., 44 White Post Lane, Victoria Park, London, Eng.	A Jan. 19, 1892 M May 17, 1892
IVES, EDWARD B.	Chief Engineer, Raritan Construction Co., 153 Bullitt Bldg., Philadelphia, Pa.	A April 2, 1889 M May 15, 1894
JACKSON DUGALD C.	Professor of Electrical Engineering, University of Wisconsin, Madison, Wis.	A May 3, 1887 M June 17, 1890
JACKSON, FRANCIS E.	Aylsworth & Jackson, Incandescent Filament Manufacturers, 128 Essex Ave , Orange; residence, 61 South Grove St., East Orange, N.J.	A Jan. 3, 1888 M June 17, 1890
JACKSON, HENRY	Telegraph Supt. and Engineer, The Lancashire & Yorkshire Railway Co., Horwich, Bolton-le Moors, Lancashire, England.	A Mar. 21, 1894 M Dec. 19, 1894

Name.	Address.	Date of Membership.
JACKSON, JOHN PRICE	Professor of Electrical Engineering, Penn. State College, State College, Pa.	A Sept. 27, 1892 M Jan. 17, 1894
JANNUS, FRANKLAND	Attorney-at-Law, Solicitor of Patents, 928-30 F. St, N. W. Washington, D. C.	A Nov. 12, 1889 M Mar. 18, 1890
JEHL, FRANCIS	Lichtenauergasse 8, Brünn, Moravia, Austria.	A June 27, 1895 M Jan. 22, 1896
JENKS, W. J.	Secretary, Board of Patent Control, 120 Broadway. New York City.	A June 8, 1887 M Nov. 1, 1887
JOHNSTON, A. LANGSTAFF	Chief Engineer, Richmond Traction Co., 1112 E. Main St., Richmond, Va.	A April 21, 1891 M April 18, 1894
JONES, FRANCIS WILEY [Life Member.]	Assistant Gen'l-Manager and Electrician, Postal Telegraph-Cable Co., 253 Broadway, New York City.	A April 15, 1884 M Oct. 21, 1884
KEITH, DR. NATHANIEL S.	Sandycroft Foundry, E. W. Co., Hawarden, near Chester, Eng.	A June 6, 1893 M Jan. 17, 1894
KIMBALL, DR. ALONZO S.	Professor of Physics, and Electrical Engineering, Worcester Polytechnic Institute, Worcester, Mass.	A Sept. 3, 1889 M Mar. 20, 1895
KINSMAN, FRANK E.	Electrical Engineer, 66 Broadway, New York City; residence, 836 Sherman Ave., Plainfield, N. J.	A Sept 27, 1892 M May 16, 1893
KNOWLES, EDWARD R. *E. E., C. E.*	150 Nassau Street, New York City: residence, 36 Cambridge Place, Brooklyn. N. Y.	A June 8, 1887 M July 12, 1887
KNUDSON, A. A.	Electrical Engineer, 688A Greene Ave., Brooklyn, N. Y.	A Dec. 6, 1887 M Jan. 3, 1888
LANGE, PHILIP A.	Superintendent Westinghouse Electric and Manufacturing Co., East Pittsburg, Pa.	A Mar. 6, 1888 M June 5, 1888
LANGTON, JOHN	Electrical Engineer, Canada Life Building, Toronto, Ont., and 72 Trinity Place, New York, N. Y.	A Mar. 6, 1888 M June 5, 1888
LA ROCHE, FRED. A.	President and General Manager, Ideal Electric Corporation, 652–660 Hudson Street: residence, 28 W. 25th St., New York.	A Sept. 19, 1894 M Nov. 20, 1895
LATTIG, J. W.	Electrical Engineer. Supt. of Telegraph and Electrical Apparatus, Lehigh Valley R. R. Co., So. Bethlehem, Pa.; residence, 335 Broad St., West Bethlehem, Pa	A June 8, 1887 M July 12, 1887
LAWSON, A. J.	Electrical Engineer, The County of London and Brush Provincial Electric Lighting Co., Ltd., 49 Queen Victoria St., London, Eng.	A Mar. 18, 1890 M June 17, 1890
LEMP, HERMANN, JR.	Electrician, 186 Allen Avenue, Lynn, Mass.	A April 2, 1889 M Feb. 21, 1893

Name.	Address.	Date of Membership.
LEONARD, H. WARD	Electrical Engineer, Ward Leonard Electric Co., 13th and Washington Sts., Hoboken, N. J.; residence, East Orange, N. J.	A July 12, 1887 M Sept. 6, 1887
LESLIE, EDWARD ANDREW	Vice-President and Manager, Manhattan Electric Light Co., Ltd., New York City; residence, 343 Hancock St., Brooklyn, N. Y.	A Jan. 16, 1895 M Feb. 17, 1898
LIEB, JOHN W., JR.	(*Manager.*) General Mgr., Edison Electric Ill. Co.; Residence, 166 West 97th St., New York City.	A Sept. 6, 1887 M Nov. 1, 1887
LIGHTHIPE, JAMES A.	District Engineer, General Electric Co., 15 First St., San Francisco, Cal.	A Feb. 21, 1894 M April 17, 1895
LLOYD, HERBERT	General Manager, Electrical Engineer and Chemist, The Electric Storage Battery Co., Drexel Bldg., Philadelphia, Pa.	A June 20, 1894 M May 21, 1895
LLOYD, JOHN E.	Assistant Chief Engineer, Philadelphia Traction Co.; residence, 2008 N. 18th, St., Philadelphia, Pa.	A Jan. 22, 1896 M Mar. 25, 1896
LLOYD, ROBERT MCA.	Electrician, 66 Broadway; residence, 1 West 39th St., New York City.	A Oct. 21, 1890 M Nov. 15, 1893
LOCKWOOD, THOMAS D., *F. I. Inst.* [Life Member.]	Electrical Engineer, and Advisory Electrician, P.O. Drawer 2, Boston, Mass.	A April 15, 1884 M Oct. 21, 1884
LOOMIS, OSBORN P.	Electrical and Consulting Engineer, Bound Brook, N. J.	A Sept. 16, 1890 M Dec. 16, 1896
LORRAIN, JAMES GRIEVE	Norfolk House, Norfolk St., London, W. C., England.	A May 16, 1891 M May 15, 1894
LOVEJOY, J. R.	General Manager, Supply Dept., General Electric Co., Schenectady, N. Y.	A April 21, 1891 M Feb. 21, 1894
MACFARLANE, ALEXANDER, *D. Sc., LL.D.*	Professor in Electrical Engineering, Lehigh University, South Bethlehem, Pa.	A Jan. 19, 1892 M May 17, 1892
MAILLOUX, C. O.	Consulting Electrical Engineer, 150 Nassau St., Telephone 3985 Cortlandt, New York City.	A April 15, 1884 M Oct. 21, 1884
MANSFIELD, ARTHUR NEWHALL	Assistant Electrician, American Telephone and Telegraph Co., 153 Cedar St., New York City.	A Dec. 20, 1893 M June 20, 1894
MARKS, LOUIS B., *M. M. E.*	Chief Electrician, The Electric Arc Light Co., 689 Broadway; residence, 51 East 67th St., New York City.	A May 20, 1890 M Jan. 16, 1895
MARKS, WILLIAM DENNIS, *Ph.B. C. E.*	President, The American Electric Meter Co., 1014 Betz Building, Philadelphia, Pa.	A Feb. 7, 1888 M May 1, 1888
MARSHALL, J. T.	Metuchen, N. J.	A Oct. 1, 1889 M Nov. 12, 1889
MARTIN, JULIUS	Electrician, 16 Oak St., Newark, N. J., Master Electrician, Equipment Dept., New York Navy Yard.	A Oct. 21, 1890 M Nov. 20, 1895

Name.	Address.	Date of Membership.
MARVIN, HARRY N.	Electrical Engineer and Manager, Marvin Electric Drill Co., Canastota, N. Y.	A April 19, 1892 M Jan. 17 1893
MAVER, WILLIAM, JR.	Electrical Expert and Consulting Electrical Eng'r, 27 Thames St., New York City; residence, 227 Arlington Ave. Jersey City, N. J.	A July 12, 1887 M April 21, 1891
MAYER, GEORGE M.	Enterprise Block, 5th Floor, 79 Fifth Ave., Chicago, Ill.	A Dec. 16, 1890 M June 20, 1894
MAYNARD, GEO. C.	Electrical Engineer, 800 H. St., N. W., Washington, D. C.	A April 15, 1884 M Dec. 9, 1888
McCAY, H. KENT	Electrical Engineer and Contractor, 106 E. German St., Baltimore, Md.	A Sept. 16, 1890 M May 19, 1891
McCLUER, C. E.	Superintendent, First District, So. Bell Telephone and Telegraph Co., P. O. Box 32, Richmond, Va.	A Mar. 21, 1893 M Jan. 17, 1894
McCROSKY, JAMES W.	Chief Engineer, La Capital Tramway Co. and Compaina de Luz y Fuerza Motriz de Cordoba, 715 Avenida de Mayo, Buenos Aires, Argentina.	A Dec. 20, 1893 M Dec. 16, 1896
McCROSSAN, J. A.	Manager and Electrician, Citizens' Telephone and Electric Co., Rat Portage, Ont.	A Oct. 18, 1893 M Dec. 18, 1895
McMEEN, SAMUEL G.	Engineer, Central Union Telephone Co., 1306 Ashland Block, Chicago, Ill.	A Dec. 18, 1895 M Dec. 16, 1896
MERSHON, RALPH D.	Electrical Engineer, Westinghouse Elec. and Mfg. Co. 120 Broadway, New York, N. Y.	A Mar. 20, 1895 M Jan. 22, 1896
METCALFE, GEORGE R.	136 Liberty Street; residence, 404 West 22d St., NewYork City.	A April 19, 1892 M Nov. 15, 1892
MILLIS, JOHN	Captain of Engineers U. S. A., The Lighthouse Board, Washington, D. C.	A July 7, 1884 M Mar. 3, 1885
MILLS, FRANK P.	Superintendent, Merced Gold Mining Co., Coulterville, Cal.	A Jan. 6, 1885 M Mar. 3, 1885
MITCHELL, JAMES	Constructing Engineer and Agent, General Electric Co., Caixa do Correio No. 954, Rio de Janeiro, Brazil.	A Sept. 25, 1895 M Mar. 25, 1896
MIX, EDGAR W.	Electrician, 12 Boulevard des Invalides, Paris, France.	A Sept. 3, 1889 M Mar. 20, 1895
MOLERA, E. J.	Civil Engineer, 606 Clay St., San Francisco, Cal.	A Jan. 16, 1892 M June 7, 1892
MOORE, D. McFARLAN	Inventor, Moore Electrical Co., 52 Lawrence St., Newark, N. J.	A Dec. 20, 1893 M June 20, 1894
MORROW, JOHN THOMAS	Supt. Electrolytic Plant, Boston and Montana Consolidated Copper and Silver Mining Co., Great Falls, Mont.	A Dec. 21, 1892 M April 18, 1894
NEILER, SAMUEL G.	Ass't Electrical Engineer, Pierce & Richardson, 1409 Manhattan Building, Chicago, Ill.	A April 18, 1894 M Dec. 18, 1895

Name.	Address.	Date of Membership.
NICHOLS, DR. EDWARD L.	Professor of Physics, Cornell University, Ithaca, N. Y.	A Oct. 4, 1887 M Dec. 6, 1887
NICHOLS, GEO P.	Partner, Geo. P. Nichols & Bro., Elec. Engineers and Contractors, 1036 Monadnock Bldg., Chicago, Ill.	A Jan. 22, 1896 M Nov. 18, 1896
NOLL, AUGUSTUS	Contracting Electrical Engineer, 8 East 17th St., Telephone, 62, 18th; New York City.	A Sept. 27, 1892 M April 18, 1893
NUNN, PAUL N.	Consulting Engineer. San Miguel Cons. Gold Mining Co., Telluride, Colo.	A April 17, 1895 M Feb. 26, 1895
O'CONNELL, JOSEPH J.	Telephone Engineer, Chicago Telephone Co., Residence, 76 Eugene St., Chicago. Ill.	A Oct. 17, 1894 M Nov. 20, 1895
O'DEA, MICHAEL TORPEY	Professor of Applied Electricity. University of Notre Dame, Notre Dame, Ind.	A June 8, 1887 M Mar 25, 1896
OUDIN, MAURICE	Electrical Engineer, General Electric Co., Schenectady. N. Y.	A June 20, 1894 M Nov. 20, 1895
PAINE, F. B. H.	Westinghouse Electric and Mfg. Co., 328 Exchange Building, Boston, Mass.	A Dec. 16, 1890 M Nov. 25, 1891
PAINE, SIDNEY B.	General Electric Co., 180 Summer St., Boston, Mass.	A June 8, 1887 M Nov. 1, 1887
PARKER, LEE HAMILTON	Ass't Engineer, Railway Dept., General Electric Co., Schenectady, N. Y.	A Aug. 5, 1895 M Dec. 16, 1896
PARKS, C. WELLMAN	1825 Fifth Ave., Troy, N. Y.	A July 12, 1887 M May 1, 1888
PARSHALL, HORACE FIELD	Electrical Engineer, British Thomson-Houston, Ltd., 38 Parliament St., Westminster, London, Eng.	A Sept. 7, 1888 M Mar. 18, 1890
PATTISON, FRANK A.	Firm of Pattison Bros, Consulting and Constructing Electrical Engineers, 136 Liberty St., New York City.	A Sept. 22, 1891 M Dec. 16, 1891
PEARSON, F. S.	Engineer. Room 811, 621 Broadway. New York City.	A Oct. 25, 1892 M Feb. 21, 1893
PEROT, L. KNOWLES	Vice President and Manager Schuylkill Valley Illuminating Co., Phœnixville, Pa.	A Mar. 15, 1892 M Dec. 18, 1895
PERRINE, FREDERIC A. C., *D. Sc.*	Professor of Electrical Engineering, Leland Stanford, Jr., University, Palo Alto, Cal.	A Sept. 16, 1890 M Dec. 16, 1890
PERRY, NELSON W., *E. M.*,	Editor *Electricity*, 136 Liberty St., New York City; residence, 650 Madison Ave., Elizabeth, N. J.	A May 17, 1892 M Mar. 21, 1893
PICKERNELL, F. A.	(*Manager.*) Chief Engineer. Amer. Tel. & Tel.Co., 153 Cedar St., New York City.	A Feb. 7, 1890 M Mar. 18, 1890
PIERCE, RICHARD H.	Pierce & Richardson, Electrical Engineers, 1409 and 1410 Manhattan Bldg, Chicago; residence, 5434 Monroe Ave., Hyde Park, Ill.	A April 18, 1893 M Dec. 20, 1893

Name.	Address.	Date of Membership.
PIKE, CLAYTON W., *B.S.*	Electrical Engineer, Faikenau Engineering Co., 711 Reading Terminal, Philadelphia, Pa.	A Dec. 16, 1891 M Oct. 25, 1892
PORTER, J. F.	Manager, Alton Railway and Illuminating Co., Alton, Ill.	A Sept. 6, 1887 M Nov. 1, 1887
POTTER, WM. BANCROFT,	Engineer Railway Dept., General Electric Co., Schenectady, N. Y.	A Jan. 22, 1896 M Mar. 25, 1896
POWELL, WILLIAM H.	Electrical Engineer, 55 Oak St., Hartford, Ct.	A June 17, 1890 M Mar. 20, 1893
PRATT, ROBERT J.	Greenbush, N. Y.	A July 12, 1887 M Sept. 6, 1887
PUFFER, WM. L.	(*Manager.*) Assistant Professor of Electrical Engineering, Mass. Institute of Technology, Boston; residence, West Newton, Mass.	A Dec. 20, 1893 M April 17, 1895
RAE, FRANK B.	Electrical Engineer, 1109 Fort Dearborn Bldg., 134 Monroe St., Chicago, Ill.	A April 15, 1884 M Oct. 25, 1892
REBER, SAMUEL	Lieut. Signal Corps, U. S. Army, Care of Chief Signal Officer U. S. A., Washington, D. C.	A Sept. 20, 1893 M Jan. 22, 1896
RECKENZAUN, FREDERICK,	Electrical Engineer, 44 Pine St., New York City.	A Mar. 6, 1888 M June 5, 1888
REIST, HENRY G.	Designing Engineer, General Electric Co., 5 South Church St., Schenectady, N. Y.	A June 17, 1890 M Dec. 19, 1894
RICE, E. WILBUR, JR.	Technical Director, The General Electric Co., Schenectady, N. Y.	A Dec. 6, 1887 M Jan. 3, 1888
RIES, ELIAS E.	Electrical Engineer and Inventor, 1031 Temple Court; residence, 4 W. 115th St., New York City.	A July 12, 1887 M Sept. 6, 1887
RIKER, ANDREW L. [Life Member.]	Electrical Engineer, The Riker Electric Motor Co., 45 York St., Brooklyn; residence, Stamford, Conn.	A Nov. 1, 1887 M Dec. 18, 1895
ROBB, RUSSELL	With Stone & Webster, 4 P. O. Square, Boston, Mass.	A Oct. 18, 1893 M May 21, 1895
ROBB, WM. LISPENARD	Professor of Physics, Trinity College, Hartford, Conn.	A Dec. 16, 1891 M Mar. 15, 1892
ROBERTS, E. P.	E. P. Roberts & Co., Electrical and Mechanical Engineers, Brainard Block, Telephone 2056, Cleveland, O.	A Jan. 6, 1885 M Feb. 3, 1885
RODGERS, HOWARD S.	Electrical Engineer, care General Electric Co., 264 W. 4th Street, Cincinnati, O.	A Sept. 27, 1892 M May 16, 1893
ROHRER, ALBERT L.	Electrical Engineer, with General Electric Co., Schenectady, N. Y.	A Nov. 1, 1887 M May 1, 1888
ROLLER, JOHN E.	Lieut. U. S. N., in charge of Inspection and Installation, U. S. Navy Yard, New York; residence, Cranford, N. J.	A Sept. 19, 1894 M May 19, 1896

Name.	Address.	Date of Membership.
ROSS, NORMAN	General Superintendent, Bullock Electric Mfg. Co., Cincinnati, O.	A Sept. 20, 1893 M Nov. 21, 1894
ROSS, ROBERT A.	Mechanical and Electrical Consulting Engineer, 17 St. John St., Montreal, P. Q.	A Sept. 27, 1892 M April 18, 1893
ROUQUETTE, WILLIAM F. B.	Proprietor, Rouquette & Co., 47 Dey St., New York City.	A Mar. 21, 1894 M Dec. 19, 1894
RYAN, HARRIS, J.	(*Vice-President.*) Professor of Electrical Engineering, Cornell University, Ithaca, N. Y.	A Oct. 4, 1887 M April 17, 1895
SALOMONS, *Sir* DAVID LIONEL, *Bart. M. A.*, [Life Member]	Engineer and Barrister, Broomhill, Tunbridge Wells, Kent, and 49 Grosvenor St., London, W. England.	A Feb. 7, 1888 M May 1, 1888
SANDS, H. S.	Consulting and Constructing Electrical Engineer, Peabody Building, Wheeling, W. Va.	A Feb. 21, 1893 M Nov. 21. 1894
SARGENT, W. D.	General Manager, N. Y. & N. J. Tel. Co., 16 Smith St., Brooklyn, N. Y.	A April 15, 1884 M Feb. 21, 1894
SCHEFFLER, FRED. A.	Stirling Boiler Co., 126 Liberty Street, New York City; residence, Passaic, N. J.	A May 16, 1893 M Jan. 26, 1896
SCHMID, ALBERT	Superintendent, Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	A Oct. 21, 1890 M April 17, 1895
SCHOEN, A. M.	Electrician, South Eastern Tariff Association, Norcross Building, Atlanta, Ga.	A Sept. 20, 1893 M Dec. 16, 1896
SCOTT, CHARLES F.	(*Manager.*) Chief Electrician, Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	A April 19, 1892 M Jan. 17, 1893
SEVER, GEORGE F.	Instructor in Electrical Engineering, Columbia University, New York City.	A Jan. 17, 1894 M May 19, 1896
SHALLENBERGER, O. B.	Consulting Electrician, Westinghouse Electric and Mfg. Co., of Pittsburg; Rochester, Pa.	A Sept. 7, 1888 M Dec. 4, 1888
SHAW, EDWIN C.	Manager, Akron General Electric Co., Akron, O.	A May 17, 1892 M Feb. 27, 1895
SHEA, DANIEL W.	Professor of Physics, Catholic University of America, Washington, D. C.	A Dec. 20, 1893 M June 20, 1894
SHEBLE, FRANKLIN	Sheble & Patton, Ltd., 1026 Filbert St., Philadelphia, Pa.	A Oct. 21, 1890 M Dec. 18, 1895
SHELDON, SAMUEL, *A. M.*, *Ph.D.*	Professor of Physics and Electrical Engineering, Polytechnic Institute, 198½ Schermerhorn, St., Brooklyn, N. Y.	A Dec. 16, 1890 M Oct. 27, 1891
SHEPARD, WM. E.	Steinway Railway Co., Long Island City, N. Y.	A Feb. 7, 1890 M Mar. 18, 1890
SHEPARDSON, GEORGE D.	Professor of Electrical Engineering, University of Minnesota, Minneapolis, Minn.	A April 21, 1891 M Jan. 22, 1896

Name	Address.	Date of Membership.
SINCLAIR, H. A.	Electrical Engineer, The Tucker Electric Co., 950 Bedford Ave., Brooklyn, N. Y.	A June 17, 1890 M Feb. 26, 1896
SMITH, FRANK STUART	Supt. Lamp Factory, Westinghouse Electric & Mfg. Co., Pittsburg, Pa.	A Sept. 27, 1892 M April 18, 1893
SMITH, JESSE M.	Expert in Patent Causes, Consulting Electrical and Mechanical Engineer, 36 Moffat Block, Detroit, Mich.	A April 15, 1884 M June 26, 1891
SMITH T. CARPENTER	Mechanical and Electrical Engineer, 212 Drexel Building, Philadelphia, Pa.	A Oct. 27, 1891 M Dec. 16, 1891
SPAULDING, HOLLON C.	Electrical Engineer, Manager, N. E. Office, Manhattan General Construction Co., 611 John Hancock Bldg., Boston, Mass.	A April 21, 1891 M June 20, 1894
SPERRY, ELMER A	Electrical Engineer, Sperry Electric Railway Co., Mason and Belden Sts., Cleveland, O.	A April 19, 1892 M Feb. 21, 1893
SPRAGUE, FRANK J	(*Past-President.*) Vice-Prest. Sprague Electric Elevator Co., Postal Telegraph Bldg., 253 Broadway and 182 West End Ave.; New York City	A May 24, 1887 M Feb. 17, 1897
STANDFORD, WILLIAM	Asst. Supt. Telegraphs, Colonial Govt., Cape Town, Cape of Good Hope, Africa.	A Oct. 4, 1887 M Dec. 6, 1887
STEARNS, CHARLES K. *A.E.*	Room 15, 116 Bedford Street, and 85 Westland Avenue, Boston, Mass.	A Aug. 6, 1889 M May 16, 1893
STEARNS, JOEL W., JR.	Treasurer, Mountain Electric Co., Box 1545, Denver, Col.	A June 20, 1894 M Nov. 20, 1895
STEBBINS, THEODORE	Engineer of Committee on Local Companies, General Electric Co., Schenectady, N. Y.	A July 9, 1889 M June 17, 1890
STEINMETZ, CHARLES P.	(*Vice-President*) Electrician, General Electric Co., Schenectady, N. Y.	A Mar. 18, 1890 M April 21, 1891
STEPHENS, GEORGE	General Supt., Canadian General Electric Co., Ltd., Peterboro, Ont.	A June 20, 1894 M Dec. 18, 1895
STIERINGER, LUTHER	Electrical Expert, Morris Building, 68 Broad St., New York City.	A June 8, 1887 M Nov. 1, 1887
STILLWELL, LEWIS B	(*Manager.*) Electrical Director, Niagara Falls Power Company, and the Cataract Construction Co., Niagara Falls, N. Y.	A April 19, 1892 M Nov. 15, 1892
STOTT, HENRY G.	Electrical Engineer, Buffalo Gen'l Electric Co., Buffalo, N. Y.	A Sept. 25, 1895 M April 22, 1896
TAINTOR, GILES	Division Sup't. Western Division New England Telephone and Telegraph Co., Springfield, Mass.	A June 26, 1891 M Dec. 16, 1891
TALTAVALL, THOS. R.	*Electrical World*, 253 Broadway, New York City.	A Jan. 20, 1891 M Oct. 27, 1891

Name.	Address.	Date of Membership.
TERRY, CHARLES A.	Lawyer, Westinghouse Electric and Mfg. Co., 120 Broadway, New York City.	A April 5, 1887 M May 17, 1887
THOMAS, BENJAMIN F., *Ph. D.*	Professor of Physics, Ohio State University, Columbus, O.	A June 7, 1892 M Nov. 15, 1892
THOMSON, PROF. ELIHU	(*Past President*). Electrician, General Electric, and Thomson Electric Welding Companies, Lynn, Mass.	A April 15, 1884 M April 21, 1891
THOMPSON, EDWARD P.	Consulting Electrician and Patent Attorney in Electrical Cases, 5 Beekman St., New York City.	A April 15, 1884 M Dec. 3, 1889
THURNAUER, ERNST	Manager, Thomson-Houston International Elec. Co., 27 Rue de Londres, Paris, France.	A Oct. 14, 1887 M Dec. 6, 1887
TISCHENDOERFER, F. W.	Electrical Engineer, Schückert & Co., Nuremberg, Germany.	A April 19, 1892 M Nov. 21, 1894
TRAFFORD, EDWARD W.	Electrical Engineer, Richmond Railway and Electric Co., Foot of 7th St., Richmond, Va.	A Feb. 21, 1894 M Dec. 19, 1894
TURNER, WILLIAM S.	Consulting and Constructing Electrical and Mechanical Engineer, 253 Broadway, New York City.	A Dec. 7, 1886 M Oct. 2, 1888
UEBELACKER, CHAS. F.	Electrical Engineer, Consolidated Traction Co., 30 North 11th St., Newark, N. J.	A Feb. 7, 1890 M Nov. 15, 1893
UHLENHAUT, FRITZ, JR.	Philadelphia Traction Co., 4101 Haverford St., Philadelphia, Pa.	A May 7, 1889 M Dec. 19, 1894
UPTON, FRANCIS R	Sales-Manager, National Tube Works Co., McKeesport, Pa.	A May 17, 1887 M Mar. 15, 1892
VAIL, J. H	Engineer-in-Chief, Penn. Heat, Light and Power Co., and Edison Electric Light Co., 909 Walnut St., Philadelphia, Pa.	A June 8, 1887 M Nov. 1, 1887
VANSIZE, WILLIAM B.	(*Manager*) Solicitor of Patents and Expert, 253 Broadway; residence, 100 W. 74th St,, New York City.	A April 15, 1884 M Oct. 21, 1884
VAN TRUMP, C. REGINALD	Engineer and Manager, Wilmington City Electric Co., Wilmington, Del.	A Feb. 5, 1886 M Feb. 21, 1894
WADDELL, MONTGOMERY	Consulting Engineer, 72 Trinity Place, New York City.	A Feb. 7, 1888 M May 1, 1888
WAIT, HENRY H.	Assistant Electrical Engineer, Western Electric Co., 4919 Madison Ave., Chicago, Ill.	A Sept. 20, 1893 M June 20, 1894
WALDO, DR. LEONARD	Electrical Engineer, Secretary, The Waldo Foundry, 57 Coleman St., Bridgeport, Conn.	A June 5, 1888 M Dec. 4, 1888
WALKER, SYDNEY F.	Electrical Engineer, 195 Severn Road, Cardiff, Wales.	A June 2, 1885 M May 17, 1887
WARING, JOHN	Ovid, N. Y.	A Dec. 16, 1890 M April 17, 1895

Name.	Address.	Date of Membership.
WARNER, ERNEST P.	Electrical Engineer, Western Electric Co.; residence, 402 Belden Ave., Chicago, Ill.	A Sept. 20, 1893 M June 20, 1894
WATERMAN, F. N.	Electrical Engineer, Westinghouse Electric and Mfg. Co., 120 Broadway, New York City.	A Feb. 21, 1893 M June 20, 1894
WEAVER, W. D.	(*Manager.*) Editor *American Electrician*, 7 West 26th Street, New York City.	A May 17, 1887 M May 17, 1887
WEBB, HERBERT LAWS	18 Cortlandt St.; residence, 253 West 42d St., New York City.	A Oct. 21, 1890 M Dec. 16, 1890
WEEKS, EDWIN R.	706 Wall St., Kansas City, Mo.	A Sept. 6, 1887 M Nov. 1, 1887
WELLER, HARRY W.	Electrical Engineer, Room 206, Equitable Building, Boston, Mass.	A Oct. 21, 1890 M Nov. 24, 1891
WESTON, EDWARD	(*Past President.*) Vice-President, Weston Electrical Instrument Co, 120 William St., and 645 High St., Newark, N. J.	A April 15, 1884 M Oct. 21, 1884
WETZLER, JOSEPH	Editor *The Electrical Engineer*, 203 Broadway, New York City.	A April 15, 1884 M Dec. 9, 1884
WHARTON, CHAS. J.	82 Bond St., London, Eng,	A Jan. 3, 1888 M May 1, 1888
WHEELER, SCHUYLER SKAATS, *Sc.D.* [Life Member.]	President, Crocker-Wheeler Electric Co., 39 Cortlandt St., N. Y., and Ampere, N. J.; residence, 4 West 33d St., New York City.	A June 2, 1885 M Sept. 1, 1885
WHITE-FRASER, GEO.	*Mem. Can. Soc. C. E.*; 18 Imperial Loan Building, Toronto, Ont.	A Sept. 22, 1891 M Dec. 18, 1895
WIENER, ALFRED E.	Electrical and Mechanical Engineer; residence, 208 Liberty St., Schenectady, N. Y.	A May 16, 1893 M May 15, 1894
WILCOX, NORMAN T.	Manager and Electrican, Seneca Light and Power Co., Seneca Falls, N. Y.	A May 21, 1895 M Jan. 22, 1896
WILKES, GILBERT	Consulting Electrical Engineer, 1112 Union Trust Building, Detroit, Mich.	A Jan. 7, 1890 M Mar. 18, 1890
WILLYOUNG, ELMER G.	E. G. Willyoung & Co., Scientific Instruments and Apparatus, 938 Market St., Philadelphia.	A Nov. 24, 1891 M Dec. 20, 1893
WILSON, CHARLES H.	Monadnock Building, Chicago. Ill.	A Nov. 24, 1891 M Feb. 16, 1892
WILSON, FREMONT	Electrician, 66 Maiden Lane, (Telephone, 1651 Cortlandt) and 2153 Seventh Ave., New York City.	A Mar. 6, 1888 M June 5, 1888
WILSON, HARRY C.	Supt. of P. O. Telegraph with the Government, Kingston, Jamaica, West Indies.	A Jan. 19, 1891 M June 7, 1892
WINCHESTER, A. E.	Consulting Engineer and Designer of Electric Systems, South Norwalk, Conn.	A June 8, 1887 M Nov. 1, 1887

Name.	Address.	Date of Membership.
WINSLOW, GEORGE HERBERT	Consulting Electrical Engineer, 700 Lewis Block, 6th Ave., and Smithfield St., Pittsburg, Pa.	A April 17, 1895 M Feb. 26, 1896
WIRT, CHARLES	Consulting Engineer, 1028 Filbert St., Philadelphia, Pa.	A Sept. 8, 1888 M June 20, 1894
WOLCOTT, TOWNSEND	Electrician, 1002 Bennett Building, New York City.	A Mar. 6, 1888 M Dec. 16, 1890
WOLVERTON, B. C.	Electrician, N. Y. & Pa. Telephone and Telegraph Co., Elmira, N. Y.	A Mar. 18, 1890 M Feb. 21, 1895
WOODBRIDGE, J. L.	Secretary and Treasurer, Woodbridge & Turner Engineering Co., 47 Times Building, New York City.	A June 8, 1887 M Nov. 1, 1887
WRIGHT, PETER	General Superintendent, People's Electric Light and Power Co., 36 Mechanic St., Newark, N. J.	A May 16, 1889 M Jan. 16, 1895
WURTS, ALEXANDER JAY	Westinghouse Electric & Mfg. Co., Pittsburg, Pa.	A April 19, 1892 M Nov. 15, 1892
YOUNG, C. GRIFFITH	Electrical Engineer, White-Crosby Co., 706 Equitable Building., Baltimore, Md.	A Jan. 3, 1889 M April 21, 1891

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ASSOCIATE MEMBERS.

Name.	Address.	Date of Election.
ABELLA, JUAN	Director General of Public Lighting, Buenos Aires; residence, 691 Calle Bolivar, Buenos Aires, Argentine Republic.	Aug. 5, 1896
ADAE, CHAS. FLAMEN	X-Ray Laboratory, P. O. Box, 2809; residence, 36 West 35th Street, New York City.	Dec. 16, 1896
ADAMS, COMFORT A., JR.	Assistant Professor of Electrical Engineering, Harvard University, 13 Farrar St., Cambridge, Mass.	Jan. 17, 1894
ADAMSON, DANIEL	Manager Joseph Adamson & Co., Hyde, Cheshire, England.	Feb. 26, 1896
AGNEW, CORNELIUS R.	Electrical Engineer, 150 Nassau St., 23 West 39th St., New York City.	Mar. 21, 1894
ALBANESE, G. SACCO	Electrical Expert, Compagnie Francaise Thomson-Houston, Mustapha, Algeria.	Sept. 20, 1893
ALBERT, HENRY	Electrical Engineer, 815 Main St., Jacksonville, Fla.	Feb. 21, 1893
ALDEN, JAMES S.	Assistant Manager, with L. H. Alden, 486 River Drive, Passaic, N. J.	May 19, 1891
ALDRICH, WILLIAM S.	Professor of Mechanical Engineering and Director Mechanical Arts, West Virginia University, P. O. Box 256, Morgantown, W. Va.	Mar. 15, 1892
ALEXANDER, HARRY	Electrical Engineer, General Manager and Vice Prest. Alexander-Chamberlain Electric Co., 56 West 22d St., and 348 W. 145th St., New York City.	April 21, 1891
ALEXANDER, P. H.	Manager, Lighting Dept., Electric Selector and Signal Co., 43 Cortlandt St., New York City.	Dec. 16, 1890
ANDERSON, HENRY S.	General Manager and Electrician, United Electric Light Co., Springfield, Mass.	Jan. 16, 1895
ANDREWS, WILLIAM, C.	Electrical Engineer, Royal Electric Co; residence, 147 Metcalfe St., Montreal, P. Q.	May 21, 1895
ANSON, FRANKLIN ROBERT	Receiver, Salem Consolidated Street Railway Co., Salem, Ore.	Feb. 27, 1895
ANTHONY, WATSON G.	Electrician, 32½ Webster St., Newark, N. J.	Feb. 24, 1891
APPLEYARD, ARTHUR E.	Manager and Engineer, Natick Gas and Electric Co., Natick, Mass.	Aug. 5, 1896
ARCHBOLD, WM. K.	Westinghouse Electric and Mfg. Co. 328 Exchange Bldg., Boston, Mass.	June 20, 1894
ARCHER, GEO. F.	Electrical Engineer, 31 Burling Slip, New York City.	Nov. 21, 1894
ARMSTRONG, CHAS. G.	Electrical Expert, 1306 Great Northern Hotel Building, Chicago, Ill.	Sept. 27, 1892

Name.	Address.	Date of Election.
ARNOLD, CRAIG R.	Electrician and Treasurer, Arnold Electric Co., Chester and Sharon Hill, Pa.	Nov. 15, 1892
ASHLEY, FRANK M.	Master Mechanic, Ashley Engineering Works, 69 Beekman St., New York.	Nov. 21, 1894
AUSTIN, SYDNEY B.	55 Franklin St.; residence, 130 West 83rd Street, New York.	Sept. 25, 1895
AUERBACHER, LOUIS J.	Auerbacher & Venino, Electrical Engineers and Contractors, 317 Market St., Newark, N. J.	Sept. 20, 1893
BABCOCK, CLIFFORD D.	[Address unknown.]	Feb. 21, 1894
BADEAU, ISAAC F.	Assistant to the Engineer, New York Telephone Co.; residence, 162 Prince St., Brooklyn, N. Y.	Feb. 26, 1896
BALDWIN, JAS. C. T.	Superintendent Bell Telephone Co., of Mo.; 10th and Olive Sts., St. Louis, Mo.	April 17, 1895
BALL, WM. D.	Consulting Electrical Engineer, W. D. Ball & Co , 1625 Monadnock Block, Chicago, Ill.	Nov. 20, 1895
BANCROFT, CHAS. F.	Electrical Engineer, Lowell and Suburban Street Railway, Lowell, Mass.	Dec. 18, 1895
BARBOUR, FRED FISKE	Manager, Power and Mining Department, Pacific District, General Electric Co., 15 First St., San Francisco, Cal., and 1673 Valdez St., Oakland, Cal.	May 16, 1893
BARNARD, JOHN H.	Interior Te'ephone Co., 203 Broadway, New York City.	June 26, 1891
BARNES, EDWARD A.	Electrical Expert, Fort Wayne Electric Co., Fort Wayne, Ind.	Sept. 20, 1893
BARRY, DAVID	Electrician and Superintendent, Amherst Gas Co., Amherst, Mass.	Aug. 5, 1896
BARSTOW, WILLIAM S.	General Supt., Edison Electric Illuminating Co., 360 Pearl St., Brooklyn, N. Y.	Feb. 21, 1894
BARTH-BARTOSHEVITCH, A.	Mechanical and Electrical Engineer, [Address unknown.]	May 16, 1893
BARTLETT, EDWARD E.	Member Firm Bartlett & Co., 23 Rose St., New York City.	June 6, 1893
BARTON, ENOS M.	President Western Electric Co., 227 South Clinton St., Chicago, Ill.	July 12, 1887
BATES, FREDERICK C.	Electrical Engineer, General Electric Co., 44 Broad St., New York City.	Jan. 20, 1891
BATES, PUTNAM A.	Student, Columbia University; residence, 113 West 72d St., New York City.	Jan. 20, 1897
BAUER, W. F.	Electrician, Reisterstown, Md.	April 15, 1890
BEAMES, CLARE F.	General Electric Co., Monadnock Block, Chicago, Ill.	May 21, 1895
BEATTIE, JOHN, JR.	Manager and Superintendent, The Beattie Battery, Zinc and Electric Co., Fall River, Mass.	Sept. 6, 1887

Name.	Address.	Date of Election.
BELL, ORA A.	Electrical Engineer, Western Electric Co., 22 Thames St., New York; residence, 921 St. Nicholas Ave., New York.	Aug. 5, 1896
BENNETT, EDWIN H., JR.	Electrician and Engineer, Diehl & Co., Elizabethport, N. J., and 17 E. 33d St., Bayonne, N. J.	June 20, 1894
BENNETT, JOHN C.	Electrician, General Electric Co., 44 Broad St., New York City.	Mar. 18, 1890
BENOLIEL, SOL. D., *B. S.*, *E. E.*, *A. M.*	Consulting and Contracting Electrical Engineer, 1327 Broadway; residence, 120 W. 35th St., New York City	Oct. 21, 1896
BENTLEY, MERTON H.	Chicago Telephone Co.; residence, 221 Scoville Ave., Oak Park, Ill.	Oct. 18, 1893
BERG, ERNST JULIUS	Engineer, General Electric Co.; residence, 53 Washington Ave., Schenectady, N. Y.	Sept. 19, 1894
BERG, ESKIL	Electrical Engineer, Gen'l Electric Co., Schenectady, N. Y.	Nov. 20, 1895
BERGHOLTZ, HERMAN	Secretary and Treasurer, Ithaca Street Railway Co., Ithaca, N. Y.	April 2, 1889
BERLINER, EMILE	Inventor, Columbia Road, between Fourteenth and Fifteenth Sts., Washington, D. C.	April 15, 1884
BERRESFORD, ARTHUR W., *B. S.*, *M. E.*	Electrician, Ward Leonard Electric Co., Hoboken, N. J.	May 15, 1894
BEST, A. T.	Electrical Engineer, Miami, Florida.	April 19, 1894
BETHELL, U. N.	General Manager, The New York Telephone Co., 18 Cortlandt St., N. Y. City.	Jan. 17, 1894
BETTS, HOBART D.	Member of Inspection Dept., The Edison Elec. Ill'm'g Co. of N. Y.; residence, Englewood, N. J.	Aug. 5, 1896
BETTS, PHILANDER 3d	Electrician, U. S. Navy Yard, Washington, D. C.	Mar. 25, 1896
BIDDLE, JAMES G.	Drexel Bldg., Philadelphia, Pa.; residence, 264 Rittenhouse St., Germantown, Pa.	Aug. 5, 1896
BIJUR, JOSEPH, *A. B.*, *E. E.* [Life Member.]	Manager, Electric Arc Light Co., 687 Broadway; residence, 172 West 75th St., New York City.	May 15, 1894
BLACK, CHAS. N.	Walker Company, 140 Winchester Ave., New Haven, Ct.	April 19, 1890
BLAKE, HENRY W.	Editor, *Street Railway Journal*, 26 Cortlandt St., New York City.	Nov. 13, 1888
BLAKE, THEODORE W.	125 Milk Street, Boston, Mass.	Sept. 20, 1893
BLANCHARD, CHARLES M.	Winterburn, Pa.	Sept. 19, 1894
BLAXTER, GEO. H.	Vice-President and General Manager, Allegheny County Light Co., Westinghouse Building, Pittsburg, Pa.	Sept. 25, 1895

Name.	Address.	Date of Election.
BLISS, GEORGE S.	Electrical Engineer, Central District and Printing Telegraph Co., Telephone Bldg., Pittsburg, Pa.	June 20, 1894
BLISS, WM. J. A.	Johns Hopkins University, Baltimore, Md.	Jan. 20, 1891
BLISS, WILLIAM L., *B. S.*, *M. M. E.*	Electrical Engineer, Consolidated Gas Co., 4 Irving Place, New York City; residence, 24 Irving Place, Brooklyn, N. Y.	Mar. 21, 1894
BLIZARD, CHARLES	Manager of New York Office, Electric Storage Battery Co.. 66 Broadway; residence, 34 W. 27th St., New York City.	Nov. 21, 1894
BOARDMAN, HARRY B.	1530 Grand Ave., Milwaukee, Wis.	Sept. 20, 1893
BOGART, A. LIVINGSTON	Electrical and Patent Expert, 22 Union Square, New York City.	July 10, 1888
BOGGS, LEMUEL STEARNS	Reed Hotel, Ogden, Utah.	Sept. 20, 1893
BOGUE, CHARLES J.	Manufacturer and Dealer in Electrical Supplies, 206 Centre St., N.Y. City.	Dec. 3, 1889
BOHM, LUDWIG K., *Ph.D.*,	Consulting Electrical and Chemical Expert, 117 Nassau St., N. Y. City.	Nov. 15, 1892
BOLAN, THOMAS V.	Supervising and Constructing Engineer, The General Electric Co., Schenectady, N. Y.; residence, 869 N. 41st St., Philadelphia, Pa.	Aug. 5, 1896
BOYLES, THOMAS D.	Electrical Engineer, General Electric Co.; residence, 58 Washington Ave., Schenectady, N. Y.	Mar. 20, 1895
BRACKETT, PROF. CYRUS F.	Princeton, N. J.	April 15, 1889
BRADDELL, ALFRED E.	Electrical Inspector, Underwriters' Association, Middle Department, 316 Walnut St., Philadelphia, Pa.	Sept. 1, 1890
BRADY, E. D. A.	Consulting and Constructing Engineer, 95 Bank St., Lock P. O. Box 132, Waterbury, Conn.	Sept. 19, 1894
BRADY, FRANK W., *M. E.*	Professor of Engineering and Physics, New Mexico College of Agriculture and Mechanic Arts, Mesilla Park, N. M.	June 20, 1894
BRADY, PAUL T.	Manager, Central N. Y. Agency, Westinghouse Electric and Mfg. Co., Syracuse, N. Y.	July 12, 1887
BRAGG, CHARLES A.	Manager Phila. Agency, Westinghouse Electric and Mfg. Co., 302 Girard Building, Philadelphia, Pa.	Sept. 20, 1893
BRAYSHAW, I.	Telegraph Inspector Great Southern Railway, City of Buenos Aires.	Aug. 5, 1896
BRIXEY, W. R.	Proprietor and Manufacturer, Day's Kerite Wire and Cables, 203 Broadway, New York City.	Sept. 20, 1893
BROICH, JOSEPH	Superintendent and Electrician, with F. Pearce, New York City; residence, 448 8th Ave. Brooklyn, N. Y.	Jan. 17, 1894

Name.	Address.	Date of Election.
BROPHY, WILLIAM	Electrician to the Wire Department, 12 Old Court House, Boston; residence, 17 Egleston St., Jamaica Plain, Mass.	Mar. 5, 1889
BROWN, ALBERT W.	Mechanical and Electrical Engineer, Room 18, 39 Cortlandt Street; residence, 27 W. 24th St., New York.	Feb. 17, 1897
BROWN, CHAS. L.	Ass't Eng. Pintsch Compressing Co., and Foreman 159th St. Plant, 160 Broadway, New York City.	Nov. 20, 1895
BUBERT, J. F.	Supervising and Contracting Electrical Engineer, 402 Exchange Bldg., (Telephone 1379) Boston, Mass.	June 7, 1892
BUCK, HAROLD W.	14 East 45th St., New York City.	Jan. 16, 1895
BUCKINGHAM, CHAS. L.	Patent Attorney, Western Union Telegraph Co., 195 Broadway, P. O. Box 856, New York City.	April 15, 1884
BUNCE, THEODORE D.	The Storage Battery Supply Co., 239 E. 27th St., New York City.	May 20, 1890
BURGESS, CHAS. FRED'K.	Inspector in Electrical Engineering, University of Wisconsin, Madison, Wis.	Mar. 25, 1896
BURKE, JAMES	Firm of Herrick & Burke, 150 Nassau St., New York City.	May 16, 1893
BURNETT, DOUGLASS, *B.S.*	Edison Illuminating Co., Inspection Dept., 55 Duane St., New York City; residence, 42 Livingston St., Brooklyn, N. Y.	Feb. 21, 1893
BURT, BYRON T.	Manager and Sec'y, and Treas. Charleston Light and Power Co., Charleston, S. C.	Sept. 25, 1895
BURTON, PAUL G.	Constructing Electrician, Western Electric Co.; residence, 164 W. 129 St., New York City.	Nov. 20, 1895
BURTON, WILLIAM C.	With White-Crosby Co., 29 Broadway, New York, N. Y.	Sept. 20, 1893
BUTLER, WILLIAM C.	President, The Puget Sound Reduction Co., Everett, Washington.	Mar. 21, 1893
BUYS, ALBERT	Electrical Engineer, The Rahway Electric Light and Power Co., Rahway, N. J.	Feb. 7, 1890
BYRNS, ROBERT A.	Walker Company, 253 Broadway, New York City; residence, 187 Carlton Ave., Brooklyn, N. Y.	Dec. 16, 1896
CABOT, FRANCIS ELLIOTT	Supt. of Inspection and Electrician, Boston Board of Fire Underwriters, 55 Kilby Street; residence, East Milton, Mass.	April 17, 1895
CABOT, JOHN ALFRED	City Electrician, 115 W. 8th St., Cincinnati, O.	May 16, 1893
CALDWELL, EDWARD	Manager, Railway Advertising Co., 261 Broadway, New York City.	Jan. 20, 1891
CALDWELL, FRANCIS C.	Assistant Professor of Electrical Engineering, Ohio State University, Columbus, O.	June 20, 1894

Name.	Address.	Date of Election.
CANFIELD, MILTON C.	Electrical Engineer, 18 Clinton St., Cleveland, O.	Feb. 21, 1893
CANFIELD, MYRON E.	Western Electric Co.; residence, 404 W. 44th St. New York City.	May 21, 1895
CAPUCCIO, MARIO	Raimondo & Capuccio, Consulting Engineers and Patent Agents, Piazza Statuto 15, Turin, Italy.	Dec. 20, 1893
CARICHOFF, E. R.	Electrical Engineer. Sprague Electric Elevator Co., Bloomfield, N. J.	Mar. 21, 1894
CARPENTER, CHAS. E.	Vice-President, Carpenter Enamel Rheostat Co.; residence, 36 W. 35th St., New York.	Aug. 5, 1896
CARSON, DAVID I.	Secy. and Gen. Supt., The Southern Bell Telephone and Telegraph Co., 26 Cortlandt St., New York City.	Dec. 21, 1892
CARTY, J. J.	Engineer, New York Telephone Co., 18 Cortlandt St., New York City; residence, Cranford, N. J.	April 15, 1890
CASE, WILLARD E.	196 Genesee St., Auburn, N. Y.	Feb. 7, 1888
CASPER, LOUIS	Electricial Engineer and Contractor, 307 New Ridge Bldg., Kansas City, Mo.	April 21, 1891
CHADBOURNE, HENRY R., JR.	Electrical Engineer, 130 Bedford St., Boston, Mass.	May 15, 1894
CHAPMAN, A. WRIGHT	Electrical Engineer, 160 Hicks St., Brooklyn, N. Y.	Mar. 25, 1896
CHENEY, FREDERICK A.	Maple Avenue, Elmira, N. Y.	Oct. 1, 1889
CHERMONT, ANTONIO LEITE	Engineer, Firm of Chermont, Silva and Miranda, Box 252, Para, U. S. Brazil.	Mar. 18, 1890
CHESNEY, C. C.	Electrician, Stanley Laboratory, Pittsfield, Mass.	June 20, 1894
CHILDS, SUMNER W.	The Degnon Construction Co., 26 Wade Bldg., Cleveland, Ohio.	May 15, 1894
CHILDS, WALTER H.	Brattleboro, Vt.	Sept. 6, 1887
CHISM, GEORGE F.	Civil Engineer, 92 State St., Albany, N. Y.	Mar. 21, 1893
CHUBBUCK, H. EUGENE	Vice-President, The Pueblo Electric Street Railway Co., Pueblo, Col.	Dec. 4, 1888
CLARK, CHAS. M.	Student, Electrical Course, Columbia University; residence, 831 Madison Ave., New York City.	April 22, 1896
CLARK, LEROY, JR.	Electrician, Safety Insulated Wire and Cable Co., 229 West 28th St., residence, 350 West 30th St., New York City.	May 15, 1894
CLARK, WILLIAM J.	General Manager, Railway Dept. General Electric Co., 44 Broad Street, New York City.	April 22, 1896
CLEMENT, LEWIS M.	1013 Central Ave., Oakland, Cal.	April 21, 1891
CLOUGH, ALBERT L.	Box 114, Manchester, N. H.	Feb. 21, 1894

Name.	Address.	Date of Election.
Cody, L. P.	Manager and Engineer, Grand Rapids Electric Co., 9 South Division St., Grand Rapids, Mich.	Aug. 5, 1896
Coffin, Chas. A.	General Electric Co., 180 Summer St., Boston, Mass.	Dec. 6, 1887
Cogswell, A. R.	Electrician and Superintendent, Halifax Illuminating and Motor Co., Ltd., 34 Bishop St., Halifax, N. S.	April 21, 1891
Coho, Herbert B.	H. B. Coho & Co., Electrical Engineers, 203 Broadway, New York City.	Mar. 21, 1894
Coles, Edmund P.	Resident Engineer, Manáos Electric Lighting Co , Manáos, U. S. Brazil.	Oct. 23, 1895
Colgate, Geo. L.	Electrical Engineer, Ironclad Rheostat Co., Westfield, N. J.; residence, Fanwood, N. J.	June 17, 1890
Colles, George W. Jr.	*A. B.*, *M E.* Metropolitan Water Board, 3 Mt. Vernon St., Boston, Mass.	Oct. 23, 1895
Collett, Samuel D.	Engineer Construction Dep't. New York Telephone Co.. 18 Cortlandt St., New York City; residence, Van Pelt Manor, N. Y.	Feb. 26, 1896
Colville, Frank C.	Electrician and Inventor, 1503 Seventh Ave.. Oakland, Cal.	May 19, 1891
Compton, Alfred G.	Professor of Applied Mathematics, College of the City of New York, 17 Lexington Ave., New York City.	Nov. 1 1887
Coolidge, Charles A.	Electrical Engineer, Superintendent, Northern Improvement Co , 591 Hood St., Portland, Ore.	April 19, 1892
Corey, Fred Brainard	Sec'y Springfield Elevator and Pump Co., Springfield. Mass.	Dec. 20, 1893
Cornell, John B.	Supt. of Construction, with Chas. L. Cornell, Hamilton, O.	Sept. 25, 1895
Corson, William R. C.	Electrical Engineer, The Eddy Electric Mfg. Co., Windsor, Conn.	Jan. 17, 1893
Cory, Clarence L.	Professor of Electrical Engineering, University of California, Berkeley, Cal.	April 19, 1892
Crain, John Jay,	Electrician's Helper, Niagara Falls Power Co., Niagara Falls, N. Y.	Dec. 16, 1896
Crandall, Chester D.	Assistant Treasurer. Western Electric Co., 227 South Clinton St.; residence, 4438 Ellis Ave. Chicago, Ill.	Sept. 27, 1892
Crane, W. F. D.	Manager Electrical Department H. W. Johns Manufacturing Co., 87 Maiden Lane, New York City; residence, 24 Halstead Pl., East Orange, N. J.	Feb. 7, 1888
Crawford. David Francis	Ass't to Supt. Motive Power, Penn'a Co., Fort Wayne, Ind.	Sept. 25, 1895
Crawford, L. G.	Sup't, Repair Dep't General Electric Co., Chicago, Ill,	Oct. 23, 1895

Name.	Address.	Date of Election.
CREAGHEAD, THOMAS J.	President and General Manager, Creaghead Engineering Co., 296 Plum St., Cincinnati, O.	Sept. 20, 1893
CREHORE, ALBERT C., *Ph.D.*	Assistant Professor of Physics, Dartmouth College, Hanover, N. H.	Dec. 21, 1892
CREWS, J. W.	Manager, Southern Bell Telephone and Telegraph Co., 124 Main St., Norfolk, Va	Sept. 19, 1894
CRIGGAL, JOHN E.	Electrician, 12 Nelson Place, Newark, N. J.	June 20, 1894
CROSBY, OSCAR T.	White-Crosby Co., 1417 G Street, Washington, D. C.	Mar. 18, 1890
CROXTON, A. L.	Electrical Engineer, Standard Electric Co., 7118 Drexel Ave., Chicago, Ill.	June 20, 1894
CUMNER, ARTHUR B.	69 Broad St., Boston, Mass.	Feb. 27, 1895
CUNNINGHAM, E. R.	Sup't Fort Dodge Light and Power Co., Fort Dodge, Iowa.	Jan. 22, 1896
CUNTZ, JOHANNES H.	Assistant to President Henry Morton, Stevens Institute of Technology, 325 Hudson St., Hoboken, N. J.	Mar. 5, 1889
CURTIS, CHAS. G.	410 Havemeyer Bldg., New York City,	April 15, 1884
DACUNHA, MANOEL IGNACIO	Manager of the Electrical Section, Emprera Industrial Gram-Para, Para, U. S. of Brazil.	May 16, 1893
DAME, FRANK L.	General Sup't, Tacoma Railway and Motor Co., Tacoma, Wash.	June 26, 1891
DANA, R. K.	Agent, Washburn and Moen Mfg. Co., 16 Cliff St., New York City.	April 15, 1884
DANIELSON, ERNST	Consulting Electrician, 16 Scheele Gatan, Stockholm, Sweden.	June 27, 1895
DARROW, ELEAZAR	Professor M. E. Dept. Washington Agr. College, Pullman, Wash.	Aug. 5, 1896
DAVENPORT, C. G.	Expert and Agent, General Electric Co., 44 Broad St., New York City.	Nov. 21, 1894
DAVENPORT, GEORGE W.	61 Ames Bldg., Boston, Mass.	June 4, 1889
DAVIDSON, EDW. C.	Patent Lawyer, Room 179 Times Bldg., New York City.	Feb 7, 1890
DAVIS, DELAMORE L.	Superintendent, Salem Electric Light and Power Co., 299 Lincoln Ave., Salem, O.	April 2, 1889
DAVIS, JOSEPH P.	Engineer, American Bell Telephone Co., 113 W. 38th St., New York City.	April 15, 1884
DAVIS, W. J., JR.	Electrical Engineer, General Electric Co., Schenectady, N. Y.	Mar. 20, 1895
DECKER, EDWARD P.	Electrical Engineer, New York Telephone Co., 18 Cortlandt St., New York City; residence, Van Pelt Manor, N. Y.	Feb. 26, 1896
DEGEN, LEWIS	Constructing Engineer, Gen'l Electric Co., Rio de Janeiro, Brazil.	Sept. 25, 1895

Name.	Address.	Date of Election.
DEKHOTINSKY, CAPT. ACHILLES.	Late Chief Electrician and Torpedo Officer, Imperial Russian Navy, American Bell Telephone Co., 42 Farnsworth St, Boston, Mass.	Oct. 27, 1891
DELANCEY, DARRAGH	Manager of Kodak Park Works, Eastman Kodak Co., Rochester, N. Y.	Sept. 19, 1894
DENTON, JAMES E.	Professor of Experimental Mechanics, Stevens Institute of Technology, Hoboken, N. J.	July 12, 1887
DESMOND, JERE. A.	Electrical Engineer, Boston Electric Light Co., Ames Bldg., Boston, Mass	Jan. 19, 1892
DEWAR, JOHN THOMAS	Electrical Expert, Western Electric Co.; residence, 33 Rue Bouewijns, Antwerp, Belgium.	May 21, 1895
DEY, HARRY E.	Pres't and Gen'l Mgr. Dey-Griswold Co., 108 Fulton St., New York City, residence, 342 Tenth St., Brooklyn, N. Y.	Dec. 19, 1894
DICKERSON, E. N.	Attorney-at-Law, 64 E. 34th St., New York City.	April 15, 1884
DOBBIE, ROBERT S.	Electrical Engineer, Riding Mill-on-Tyne, Northumberland, Eng.	Feb. 5, 1889
DOOLITTLE, CLARENCE E.	Manager and Electrician, Roaring Fork Electric Light and Power Co., Aspen, Colo.	May 15, 1894
DOOLITTLE, THOMAS B.	Engineering Department, American Bell Telephone Co., 125 Milk St., Boston, Mass.	May 16, 1893
DOREMUS, CHARLES A.	*M.D. Ph.D.* 59 W. 51st St., New York City.	July 7, 1884
DORR, FRANK H.	Electrical Engineer, General Electric Co., Monadnock Building, Chicago, Ill.	May 15, 1894
DRESSLER, CHARLES E.	17 Lexington Ave., New York City.	Dec 16, 1890
DRYSDALE, WILLIAM A.	Consulting Electrical Engineer, Hale Building, Philadelphia, Pa.	Sept. 19, 1894
DU BOIS, JULIAN	Chief Electrician, Mohawk Division N. Y. C. & H. R. R. R. Albany, N. Y.	Nov. 20, 1895
DUNCAN, JOHN D. E.	333 S. 4th St., Terre Haute, Ind.	Mar. 20, 1895
DUNCAN, THOMAS	Electrician, Laboratory Fort Wayne Electric Corporation, 407 Broadway, Fort Wayne, Ind.	Oct. 17, 1894
DUNLAP, WILL KNOX	Electrical Engineer, Westinghouse Elec. and Mfg. Co., Niagara Falls, N. Y.	Sept. 25, 1895
DUNN, KINGSLEY G.	Dunn & McKinley, Electrical Contractors, 523 Mission St., San Francisco, Cal.	Oct. 17, 1894

Name.	Address.	Date of Election.
DURANT, EDWARD	Electrician, 115 East 26th St., New York City.	Nov. 15, 1892
DURANT, GEO. F.	Vice-Pres't Bell Telephone Co., of Mo., 511 No. 4th St., St. Louis, Mo.	April 15, 1884
DYER, FRANCIS MARON	Associate Engineer with Chas. L. Eidlitz, 10 West 23d St ; residence, 355 Lenox Ave., New York City.	Sept. 19, 1894
EDDY, H. C.	Electrical Engineer and Contractor, Lees Building, Chicago, Ill.	June 20, 1894
EDEN, MORTON EDWARD	Electrical Inspector, Western District the Underwriters' Association of the Middle Department, 245 Fourth Ave., Pittsburg ; residence, Warren Pa.	Sept. 19, 1894
EDWARDS, JAMES P.	Electrical Engineer, 1569 Walton Way, Augusta, Ga.	April 19, 1892
EGLIN, WM. C. L.	Chief of Electrical Department, Edison Electric Light Co., 909 Walnut St.; residence, 4230 Chester Ave., Philadelphia, Pa.	Sept. 19, 1894
EIDLITZ, CHAS. L.	10 West 23d St.: residence, 1125 Madison Ave , New York City.	Sept. 19, 1894
EKSTROM, AXEL	Electrical Engineer, General Electric Co.; Schenectady, N. Y.	June 17, 1890
ELEY, HARRIS H.	Electrical Workshop Supt. W. C. & S. W. Telephone Co., 88 Colston St., Bristol, Eng.	Jan. 7, 1890
ELLICOTT, EDWARD B.	Superintendent of Construction, Western Electric Co., 4438 Ellis Ave., Chicago, Ill.	Sept. 19, 1894
ELMER, WILLIAM, JR.	Electrical Engineer, Trenton Iron Co., Trenton, N. J.	Mar. 18, 1890
ELY, WM. GROSVENOR, JR.	8 Union Street, Schenectady, N. Y.	Mar. 21, 1893
EMMET, HERMAN L. R.	Publisher and Printer, 36 Cortlandt St., New York City.	April 15, 1884
ENDE, SIEGFRIED H.	1459 Madison Ave., New York City.	Jan. 17, 1894
ENTZ, JUSTUS BULKLEY	Electrical Engineer, Electric Storage Battery Co., 19th St., and Allegheny Ave., Philadelphia, Pa.	Jan. 7, 1890
ERICKSON, F. WM.	Edison Electric Illuminating Co., 3 Head Place, Boston, Mass.	Sep. 19, 1894
ESSICK, SAMUEL V.	Electrician, Consolidated Telegraph and News Co., 53 Park Place, New York; residence, Yonkers, N. Y.	May 19, 1891
ESTY, WILLIAM	Assistant Professor of Electrical Engineering, State University, Urbana, Ill.	Mar. 20, 1895
ETHERIDGE, LOCKE	Chicago Telephone Co.; residence, 4714 Kenwood Ave., Chicago, Ill.	Oct. 17, 1894
ETHERIDGE, E. L.	Care of J. P. Hall, 143 Liberty St., New York City	Dec. 20, 1893

Name.	Address.	Date of Election.
EVANS, EDWARD A.	Acting Chief Engineer, The Quebec, Montmorency and Charlevoin Railway, Quebec, Canada.	Jan. 22, 1896
EYRE, M. K.	Manager Harrison Works, General Electric Co., Harrison, N. J.	Oct 17, 1894
FARNSWORTH, ARTHUR J.	Chief Engineer, Larchmont Electric Co., Mamaroneck, N. Y.	Jan. 16, 1895
FAY, THOMAS J.	Crocker-Wheeler Electric Co., 143 Liberty St., New York City.	June 26, 1891
FIELDING, FRANK E. (Life Member.)	Chemist and Assayer, Virginia City, Nev.	Sept. 6, 1887
FIRTH, WM. EDGAR	Chief Engineer, The Midvale Steel Co., Nicetown, Philadelphia; residence, 7203 Boyer St., Germantown, Pa.	Mar. 25, 1896
FISCHER, GUSTAVE J.	Engineer for Tramway Construction, Public Works Department, Sydney, N. S. W.	Jan. 20, 1891
FISH, MILTON L.	606 Nordyke Ave., Indianapolis, Ind.	Oct. 21, 1896
FISHER, HENRY W.	Electrician and Director of Elec. and Chem. Laboratories; The Standard Underground Cable Co., Pittsburg, Pa.	Jan. 16, 1895
FISKE, J. PARKER B.	164 Devonshire St., Boston, Mass.	June 17, 1890
FLAGG, STANLEY G., JR.	Stanley G. Flagg & Co., 19th St., and Penna. Ave., Philadelphia, Pa.	April 18, 1893
FLANAGAN, THOMAS FRANCIS	General Manager, Suburban Light and Power Co., 32 Hawley Street, Boston, Mass.	Jan. 17, 1894
FLATHER, JOHN J.	Professor of Mechanical Engineering, Purdue University, Lafayette, Ind.	April 19, 1892
FLEGEL, GEO. C.	East Chicago, Ind.	Sept. 20, 1893
FLEMING, RICHARD	Electrician, Navy Yard, N. Y.; residence, Jamaica, N. Y.	Oct. 18, 1893
FLESCH, CHARLES	Electrical Engineer, Allgemeine Elektricitats Gesellschaft, 22 Schiffbauerdamm, Berlin, N. W. Germany.	Sept. 27, 1892
FLINT, BERTRAM P.	Electrical and Mechanical Engineer, Supt. Washington, Alexandria and Mt. Vernon Electric Railway Co., Washington, D. C.	Jan. 17, 1894
FLOOD, J. F.	Sup't Steubenville Traction Co., Steubenville, O.	Mar. 18, 1890
FLORY, CURTIS B.	Link Belt Engineering Co., Nicetown, Philadelphia, Pa.	April 22, 1896
FLOY, HENRY	Engineer Westinghouse Electric and Mfg. Co., 171 La Salle St.; residence, 5540 Cornell Ave., Chicago, Ill.	May 17, 1892
FOOTE, ALLEN R.	*American Exporter*, 1144 Broadway, New York City.	April 21, 1891
FOOTE, CHARLES W.	General Manager, Citizens Traction Co., San Diego, Cal.	Sept. 22, 1891

Name.	Address.	Date of Election.
FOOTE, THOS. H.	Electrical Engineer, C. & C. Electric Co., Garwood, near Westfield, N. J.	April 21, 1891
FORBES, FRANCIS	Lawyer, 32 Nassau St., New York City.	Sept. 16, 1890
FORBES, GEORGE	Electrical Engineer, 34 Great George St., London, Eng.	Feb. 21, 1894
FORD, FRANK R., *M. E.*	Consulting Engineer, Ford, Bacon & Davis, 220 Broadway, New York City.	Mar. 25, 1896
FORD, WM. S.	Assistant to Chief Engineer, The American Bell Telephone Co., Room 73, 125 Milk St., Boston, Mass.	June 7, 1892
FRANCISCO, M. J.	President and General Manager, Rutland Electric Light Co., Rutland, Vt.	June 17, 1890
FRANKENFIELD, BUDD	Instructor in Electrical Engineering, The University of Wisconsin; residence, 640 State St., Madison, Wis.	Feb. 17, 1897
FRANKLIN, W. S.	Prof. of Physics, Iowa State College, Ames, Iowa.	Jan. 22, 1896
FRANTZEN, ARTHUR	Electrical Engineer and Contractor, 225 Dearborn St., Chicago, Ill.	Feb. 21, 1894
FRENCH, PROF. THOMAS, JR. *Ph.D.*	Avondale, Cincinnati, O.	Sept. 20, 1893
FRENYEAR, THOMAS C.	Westinghouse Electric and Mfg. Co., Erie County Bank Bldg., Buffalo, N. Y.	Sept. 25, 1895
FRIDENBERG, HENRY LESLIE, *M. E.*	Stanley Mfg. Co., (Meter Dept.,) Pittsfield, Mass.	Jan. 16, 1895
FRIEDLAENDER, EUGENE	Electrician, Carnegie Steel Company, Duquesne, Pa.	Nov. 20, 1895
FROST, FRANCIS R.	Westinghouse Electric and Mfg. Co., 427 South Ave., Wilkinsburg, Pa.	Dec. 20, 1893
FROST, JOSEPH W.	Secretary, National Automatic Fire Alarm, 335 Broadway, New York City.	Mar. 20, 1895
GALLAHER, EDWARD B.	Consulting and Supervising Engineer, 99 Cedar St.; residence, 1190 Madison Ave., New York City.	Jan. 19, 1895
GALLETLY, J. FRED.	Electrician, Swift & Co., Chicago, Ill.	Mar. 21, 1894
GARRELS, W. L.	4531 West Pine Boulevard, St. Louis, Mo.	Mar. 20, 1895
GERRY, JAMES H.	Superintendent, The Self-Winding Clock Co., 163 Grand Ave., Brooklyn, N. Y.	April 18, 1894
GERSON, LOUIS JAY	Engineer and Contractor, 712 Sansom St.; residence, 637 S. 49th St., Philadelphia, Pa.	Sept. 19, 1894
GHERARDI, BANCROFT, JR.,	Assistant in the Engineering Dept. New York Telephone Co.; residence, 30 East 33d St., N. Y. City.	June 27, 1895
GILLILAND, E. T.	Pelham Manor, N. Y.	April 15, 1884

Name.	Address.	Date of Election.
GILMORE, LUCIEN H.	Prof. of Physics and Electrical Engineering, Throop Polytechnic Institute, Pasadena, Cal.	Mar. 20, 1895
GITHENS, WALTER L.	Manager, H. P. Elec. Light and Power Co., 7284 So. Chicago Ave.; residence, 5101 Kimbark Ave., Chicago, Ills.	Jan. 22, 1896
GLADING, FRANK W., *M. E., M. S.*	With Wm. Cramp & Sons, Ship and Engine Building Co., Philadelphia, Pa.	May 15, 1894
GLADSTONE, JAMES WM.	Manager, Edison Mfg. Co., 110 East 23d St.; residence, West Orange, N. J.	April 18, 1894
GODDARD, CHRIS. M.	Secretary and Electrician, New England Insurance Exchange. Sec'y Underwriters' National Electric Ass'n, 55 Kilby St,, Boston, Mass.	April 22, 1896
GOLDMARK, CHAS. J.	Electrical Engineer, 39 Cortlandt St., and 473 Park Ave., New York City.	June 5, 1888
GOLDSBOROUGH, WINDER ELWELL, *M. E.*	Professor of Electrical Engineering, Purdue University, Lafayette, Ind.	Mar. 21, 1893
GORTON, CHARLES	Civil Engineer, Belmont, N. Y.	Nov. 12, 1889
GORDON, REGINALD	Tutor in Physics, Columbia College, residence, 339 Lexington Ave., New York City.	Feb. 24, 1891
GORRISSEN, CH.	With Siemens & Halske, Franklinstrasse 29, Charlottenburg, Ger.	Mar. 25, 1896
GOSSLER, PHILIP G.	Electrical Engineer, Royal Electric Co. 94 Queen St., Montreal, P. Q.	June 20, 1894
GOTT, CLARENCE P.	Chief Engineer and Electrician Grand Central Palace; residence, 83 Washington Place, New York City.	Nov. 20, 1895
GRAHAM, GEORGE WALLACE	80 Decatur St., Brooklyn, N. Y.	Dec. 19, 1894
GRANBERY, JULIAN H.	Draughtsman, with Post & McCord, 289 4th Ave., N. Y.; residence, Closter, N. J.	Aug. 5, 1896
GREENLEAF, LEWIS STONE	Electrical Expert, The American Bell Telephone Co., 42 Farnsworth St.; residence, "The Ludlow," Clarendon St., Boston, Mass	Aug. 5, 1896
GREEN, ELWIN CLINTON	Testing Department and Installing Work, Jenney Electric Motor Co., 206 South East St., Indianapolis, Ind.	Mar. 25, 1896
GRIFFES, EUGENE	Senior Partner, Firm of Griffes and Sumner, 506 South Broadway, Los Angeles, Cal.	Feb. 26, 1896
GRIFFIN, CAPT. EUGENE	First Vice-President, General Electric Co., Schenectady, N. Y.; residence, 323 State St., Albany, N. Y.	Feb. 7, 1890
GRIST, JAMES E.	Mechanical Engineer, Pennsylvania Iron Works Co., 50th and Lancaster Ave.; residence, 918 North 44th St., Philadelphia, Pa.	Mar. 20, 1895

Name.	Address.	Date of Election.
GROSS, S. ROSS	Electrician, Tennessee Coal, Iron and R.R. Co., Ensley, Ala.	May 17, 1892
GROWER, GEORGE G.	Electrician and Chemist, Ansonia Brass and Copper Co., Ansonia, Conn.	Mar. 18, 1890
GUY, GEORGE HELI	Secretary, The New York Electrical Society, 203 Broadway, New York City.	May 16, 1893
HADLEY, ARTHUR L.	Assistant Electrician to Chief Electrician and Gen'l Supt., Fort Wayne Electric Corporation, 149 Griffith St., Fort Wayne, Ind.	Oct. 17, 1894
HADLEY, WARREN, B.	30 Cortlandt St., New York City.	June 26, 1891
HADLEY, FRED'K W.	Electrical Eng'r, Arlington Heights, Mass.	Aug. 5, 1896
HAKONSON, CARL HAROLD	Electrical Engineer, with the Union Elektricitäts Gesellschaft, Hollmann Str. 32, Berlin S. W., Ger.	Sept. 25, 1895
HALL, EDWARD J.	Vice-President and General Manager, American Telephone and Telegraph Co., 18 Cortlandt St ,New York City.	April 18, 1893
HALL, EDWIN H.	Assistant Professor of Physics, Harvard College, Gorham St., Cambridge, Mass.	Sept. 3, 1889
HALL, J. P.	Electrical Contractor, 143 Liberty St., N. Y.; residence, 200 W. 136th St., N. Y.	Aug. 5, 1896
HALL, WILLIAM P.	President, The Hall Signal Co., Vice-President The Johnson Railroad Signal Co., 44 Broad St., New York City.	Sept. 16, 1890
HALSEY, WILLIAM B.	Electrician and Horologist, 246 Elton St., Brooklyn, N. Y.	Mar. 18, 1890
HAMERSCHLAG, ARTHUR A.	Electrical Expert, and Owner Hamerschlag & Co., 26 Liberty St., New York City.	Mar. 25, 1896
HAMMATT, CLARENCE S.	Supt., Jacksonville Electric Light Co., Jacksonville, Fla.	Sept. 20, 1893
HAMMER, EDWIN W.	Electrical Engineer, 46 Second Ave., Newark, N. J.	Nov. 18, 1896
HANCHETT, GEO. T.	Electrical and Technical Engineer, 253 Broadway, N. Y.; residence, Hackensack, N. J.	May 19, 1896
HANCOCK, L. M.	P. O. Box 151. Nevada City, Cal.	May 19, 1891
HARDING, H. McL.	253 Broadway, New York City.	May 24, 1887
HARRIS, GEORGE H.	Electrical Engineer, Birmingham Railway and Electric Co., Birmingham, Ala.	June 20, 1894
HARRIS, W. C., JR.	Electrician, Harris & Williamson, Birmingham, Ala.	April 17, 1895
HART, FRANCIS R.	President and General Manager, Cartagena-Magdalena Railway Co., care of Old Colony Trust Co., 1 Court St., Boston, Mass.	April 21, 1891

Name.	Address.	Date of Election
HARTMAN, HERBERT T.	Cor. 10th and Sansom Streets, Philadelphia, Pa.	Mar. 21, 1893
HARVEY, ROBERT R. [Life Member.]	10 So. Franklin St., Wilkes-Barre, Pa.	Sept. 25, 1895
HATHAWAY, JOSEPH D., JR.	Assistant in Cable Dep't Western Electric Co., 22 Thames St., N. Y. City.	Aug. 5, 1896
HATZEL, J. C.	Electrical Engineer and Contractor, 114 Fifth Ave., New York City.	Sept. 3, 1889
HEALY, LOUIS W.	Mechanical Engineer's Office, Altoona, Pa.	June 26, 1891
HEDENBERG, WM. L.	Firm of Hedenberg & Kinsey, Consulting and Constructing Engineers, 108 Fulton St.; residence, 83 Clinton Place, New York City.	Nov. 21, 1894
HENDERSON, HENRY BANKS	Graduate Student Cornell University, 686 Willoughby Ave., Brooklyn, N. Y.	May 21, 1895
HERMESSEN, JOHN LOUIS	Chief of Data Dept., Union Elektricitats Gesellschaft, Kleist Strasse 29, Berlin, Germany.	Jan. 20, 1897
HESSENBRUCH, GEORGE S.	Student at the College at Berlin, Berliner Str. 75, Charlottenburg, Germany.	June 27, 1895
HEWITT, CHARLES E.	Electrician, Hyer-Sheehan Electric Motor Co., 100 Johnson St., Newburgh, N. Y.	Sept. 25, 1895
HEWITT, WILLIAM R.	Superintendent, Fire Alarm and Police Telegraph, 9 Brenham Place, San Francisco, Cal.	May 15, 1894
HEWLETT, EDWARD M.	Electrical Engineer, Railway Dept. General Electric Co., Schenectady, N. Y.	May 19, 1891
HILL, GEORGE, C.E.	Consulting Engineer, 44 Broadway, New York City.	April 19, 1892
HILL, NICHOLAS S., JR.	Chief Engineer Water Department, City Hall, Baltimore, Md.	Aug. 5, 1896
HOBART, HENRY M.	Engineer, care British Thomson-Houston Co., 83 Cannon St., London Eng.	April 18, 1894
HOCHHAUSEN, WILLIAM	Electrician, 74 Hanson Pl., Brooklyn, N. Y.	April 15, 1884
HOLBERTON, GEORGE C.	Electrical and Mechanical Engineer, Box 2406, San Francisco, Cal.	May 15, 1894
HOLT, MARMADUKE BURRELL	Mining and Electrical Engineer, 287 Lexington Ave. New York, N.Y.	April 15, 1890
HOMMEL, LUDWIG	Supt. of Construction, Standard Underground Cable Co., Westinghouse Building, Pittsburg, Pa.	Jan. 20, 1897
HOOD, RALPH O.	Electrical Engineer, Danvers, Mass.	April 18, 1894
HOPKINS, NEVIL MONROE	Electrical Engineer, 1730 I Street, Washington, D. C.	Nov. 20, 1895

Name.	Address.	Date of Election.
HORNSBY, HARRY H.	Electrical Inspector, 16 City Hall, Chicago, Ill.	June 27, 1895
HOWSON, HUBERT	Patent Lawyer, 38 Park Row, New York City.	June 8, 1887
HUBBARD, ALBERT S.	Electrical and Mechanical Engineer, The Electro Chemical Storage Battery Co., Belleville, N. J.	Nov. 20, 1895
HUBBARD, WILLIAM C.	Engineering Department, The Electric Arc Light Co., 689 Broadway, New York City; residence, 427 West 7th St., Plainfield, N. J.	April 18, 1894
HUBLEY, G. WILBUR	Electrical Engineer, Louisville Electric Light Co.; residence, Kenton Club, Louisville, Ky.	Sept. 19, 1894
HUBRECHT, DR. H. F. R.	Director, Nederlandsche Bell Telephone Co., Amsterdam, Holland.	Oct. 4, 1887
HUDSON, JOHN E.	President, The American Bell Telephone Co., 125 Milk St., Boston, Mass.	Dec. 20, 1893
HUGGINS, N. W.	Salesman, etc., General Electric Co., Seattle, Wash.	Aug. 5, 1896
HUGUET, CHAS. K.	Electrical Engineer, 693 W. Adams St., Chicago, Ill.	June 27, 1895
HULL, S. P.	Chief Electrician of Hudson Div. N. Y. C. & H. R. R. R. Co., Poughkeepsie, N. Y.	May 19, 1896
HULSE, WM. S.	Electrical Engineer, Fort Wayne Electrical Corporation, 228 Fairfield Ave., Fort Wayne, Ind.	Mar. 25, 1896
HUMPHREY, HENRY H.	Consulting Electrical Engineer, Bryan & Humphrey, Turner Building, St. Louis, Mo.	Dec. 16, 1896
HUMPHREYS, C. J. R.	Manager, Lawrence Gas Co., and Edison Electrical Ill. Co., Lawrence, Mass.	Sept. 6, 1887
HUNT, ARTHUR L.	Electrician, W. R. Fleming & Co., 203 Broadway, New York City.	Sept. 19, 1894
HUNTLEY, CHAS. R.	General Manager, Buffalo General Electric Co, 40 Court St., Buffalo, N. Y.	Sept. 25, 1895
HUTCHINSON, FREDERICK L.	Electrical Engineer, Elizabeth, N. J.	June 20, 1894
IDELL, FRANK E.	Havemeyer Building, 26 Cortlandt St., New York City.	July 12, 1887
IHLDER, JOHN D.	Electrical Engineer, Otis Electric Co., Yonkers, N. Y.	Oct. 2, 1888
IIJIMA ZENTARO,	In charge of Transformer Testing Dep't., Wagner Elec. Mfg. Co., 2017 Locust St., St. Louis, Mo.	Jan. 22, 1896
INGOLD, EUGENE	Consulting Engineer and Expert, 1669 Second Ave., Pittsburg, Pa.	April 18, 1894
INSULL, SAMUEL	President, Chicago Edison Co., 139 Adams St., Chicago, Ill.	Dec. 7, 1886

Name.	Address.	Date of Election.
IRVINE, DREW W.	Box 167, Montgomery, Ala.	Sept 25, 1895
IWADARE, KUNIHIKO	Electrician, 19 Second St., Nakanoshima, Japan.	Sept. 20, 1893
IZARD, E. M.	Electrical Engineer, Room 1409, 315 Dearborn St., Chicago, Ill.	Mar. 5, 1889
JAEGER, CHARLES L.	Inventor, Maywood, N. J.	Dec. 20, 1893
JACKSON, THEODORE K.	137 56th Street, Chicago, Ill.	May 21, 1895
JACKSON, WM. STEELL	326 St. Paul St., Baltimore, Md.	April 22, 1896
JOHNSTON, W. J.	*The Electrical World*, 253 Broadway, New York City.	April 15, 1884
JONES, ARTHUR W.	Care of Gibbs, Bright & Co., Melbourne, Australia.	Oct. 17, 1894
JONES, F. R.	Professor of Machine Design, University of Wisconsin, Madison, Wis.	May 20, 1890
JONES, G. H.	Agent, General Electric Co., Casilla 18 D Santiago; residence, Iquiqui, Chili.	April 17, 1895
JONES, HENRY C.	Member of Firm, the Electric Construction and Supply Co., Montgomery, Ala.	Mar. 20, 1895
JUDSON, WM. PIERSON	U. S. Civil Engineer, Oswego, N. Y.; temporary address, U. S. Engineers Office, Buffalo, N. Y.	June 8, 1887
KAMMEYER, CARL E.	Electrical Engineer, Maywood Ill.	Sept. 19, 1894
KEEFER, EDWIN S.	Supt. of Electric Light Construction, Western Electric Co., 22 Thames St., New York City; residence, Elizabeth, N. J.	April 18, 1894
KEILHOLTZ, P. O.	U. S. Electric Power and Light Co., Holliday and Centre Sts., Baltimore, Md.	Mar. 21, 1893
KELLER, E. E.	Vice-Prest. and General Manager, Westinghouse Machine Co., 224 Murtland Ave., Pittsburg, Pa.	Sept. 20, 1893
KELLER, EDWIN R., *M.E.*	Mechanical and Electrical Engineer, Falkenau Engineering Co., Ltd., 711 Reading Terminal, 4823 Springfield Ave., Philadelphia, Pa.	Mar. 21, 1894
KELLOGG, JAMES W., *M.E.*	General Electric Co., Lighting Dept., Schenectady, N. Y.	June 26, 1891
KENAN, WM. R. JR.	Chemist and Electrical Engineer, Australian Carbide Co., Sydney, N. S.W.	Jan. 20, 1897
KENNELLY, ARTHUR E. [Life Member]	(*Manager.*) Electrician, Firm of Houston & Kennelly, 1105-1106 Betz Bldg.; residence, The Landsowne, 41st St. and Elm Ave., Philadelphia, Pa.	May 1, 1888
KER, W. WALLACE	Instructor of Electricity, Hebrew Technical Institute, 36 Stuyvesant St., New York City. Residence, 43 Waverly St., Jersey City, N. J.	Sept. 25, 1895

Name.	Address.	Date of Election.
KING, VINCENT C., Jr.	With V. C. & C. V. King, 517 West St.; residence, 110 East 16th Street, New York.	Aug. 5, 1896
KIRKEGAARD, GEORG	Mechanical and Electrical Engineer, 28 State Street, New York City; residence, Giffords, Staten Island, N. Y.	Sept. 20, 1893
KIRKLAND, JOHN W.	Electrical Engineer, General Electric Co., Schenectady, N. Y.	Mar. 21, 1894
KITTLER, DR. ERASMUS	Professor at the Technical High School, Darmstadt, Germany.	Dec. 16, 1896
KLINCK, J. HENRY	Dept. Electrical Engineering, Lehigh University, South Bethlehem, Pa.	Jan. 16, 1895
KNOX, FRANK H.	15 Esplanade St., Allegheny, Pa.	June 20, 1894
KNOX, GEO. W.	Electrical Engineer, Chicago City Railway Co., 2020 State St., Chicago, Ill.	Nov. 18, 1896
KNOX, JAMES MASON	Student in Electrical Engineering, Columbia University, School of Mines; residence, 32 West 129th St., New York City.	Jan. 17, 1894
KREIDLER, W. A.	Editor and Publisher, *Western Electrician*, 510 Marquette Building, Chicago, Ill.	Oct. 4, 1887
LABOUISEE, JOHN PETER	24 Front St., Schenectady, N. Y.	Aug. 5, 1896
LAMB, RICHARD	Chief Engineer, in charge business of the Lamb Electrical Cableways, The Trenton Iron Co., No. 1 Broadway; residence, 72 W. 69th St., New York.	Dec. 18, 1895
LAND, FRANK	The Hamilton, E. Genesee Street, Syracuse, N. Y.	Sept. 22, 1891
LANE, VANCE	Manager and Superintendent Construction, Nebraska Telephone Co., Omaha, Neb.	Dec. 19, 1894
LANPHEAR, BURTON S.	Instructor in Electrical Engineering, Maine State College, Orono, Me.	Jan. 16, 1895
LANMAN, WILLIAM H.	Board of Patent Control, 120 Broadway, New York City.	June 6, 1893
LARDNER, HENRY ACKLEY	Instructor in Electrical Engineering, State College, Penn.	Dec. 19, 1894
LARNED, SHERWOOD J.	Electrical Engineer, Chicago Telephone Co., 203 Washington St., Chicago, Ill.	Oct. 17, 1894
LARRABEE, ROLLIN N.	Western Electric Co., 242 Jefferson St., Chicago, Ill.	Mar. 20, 1895
LATHAM, HARRY MILTON	Member of Engineering Staff, Crocker-Wheeler Electric Co., Ampere, N. J.	Dec. 16, 1896
LAWTON, W. C.	Roselle, N. J.	June 6, 1893
LEBLANC, CHARLES	Ingenieur en Chef, de la Compagine Generale de Traction, 24 Boulevard des Capucines, Paris, France.	April 17, 1895

Name.	Address.	Date of Election.
LECONTE, JOSEPH NISBET	Instructor in Electrical Engineering, State University, Berkeley, Cal.	Feb. 27, 1895
LEDOUX, A. R., *M. S.*, *Ph.D.*,	9 Cliff St., New York City.	Dec. 7, 1886
LEE, JOHN C.	Chemist and Electrician, American Bell Telephone Co., Mountfort St., Longwood, Brookline, Mass.	Mar. 18, 1890
LEMON, CHARLES,	Hon. Sec'y for New Zealand for the Institution of Electrical Engineers, Palmerston, North, New Zealand.	Jan. 22, 1896
LENZ, KARL	Draughtsman, Brooklyn Union Gas Co., 400 Douglas Street, Brooklyn, N. Y.	May 19, 1896
LENZ, CHARLES OTTO	Electrical Engineer, Room 510, Industrial Trust Bldg., Providence, R. I.	Mar. 15, 1892
LE PONTOIS, LEON.	Electrical Engineer, The Westinghouse Elec. and Mfg. Co., Pittsburg, Pa.	Dec. 18, 1895
LEVIS, MINFORD	Superintendent and Electrical Engineer, Novelty Electric Co., 54 North 4th St., Philadelphia, Pa.	Feb. 21, 1893
LEVY, ARTHUR B.	Assistant Engineer, Arc Light Dept., General Electric Co., 810 Lexington Ave., New York City.	Jan. 20, 1891
LEWIS, HENRY FREDERICK WILLIAM,	Redlands, 48 Sydenham Road, Croydon, Surrey, England.	Mar. 5, 1889
LIEBIG, GUSTAV A., JR.	Tarrytown, N. Y.	Mar. 6, 1888
LILLEY, L. G.	Electrical Inspector, Underwriters' Association of Cincinnati, S. W. Cor. 3d and Walnut Sts., Cincinnati, O.; residence, Wyoming, O.	June 20, 1894
LINCOLN, PAUL M.	Electrician-in-charge, Cataract Construction Co., Niagara Falls, N. Y.	Sept. 25, 1895
LINDNER, CHAS. T.	Martin & Lindner, Electrical Engineers, Luning Building, San Francisco, Cal., residence, Berkeley, Cal.	Dec. 20, 1893
LINDSAY, WM. E.	Chief Engineer, St. Louis Dressed Beef and Provision Co., 3919 Papin St., St. Louis, Mo.	April 17, 1895
LITTLE, C. W. G.	Engineer, British Thomson-Houston Co., 38 Parliament Street, London, Eng.	April 22, 1896
LOEWENHERZ, HERMAN	Electrical and Mechanical Engineer, 1376 Lexington Ave., New York City.	Feb. 27, 1895
LORIMER, GEO. WM.	Superintendent of Construction, The Callender Telephone Exchange Co., Brantford, Canada.	Aug. 5, 1896
LORIMER, JAMES HOYT	Electrical Engineer. The Callender Telephone Exchange Co, Troy, O.	Aug. 5, 1896
LOW, GEORGE P.	Editor and Proprietor, *Journal of Electricity*, San Francisco, Cal.	Jan. 17, 1893
LOZIER, ARTHUR DE LA M. *M. E.*	Salesman and Expert, Westinghouse, Church, Kerr & Co., 26 Cortlandt St.; residence, Hotel Winthrop, 125th St., W., New York City.	Mar. 25, 1896

Name.	Address.	Date of Election.
LOZIER, ROBERT T. E.	Electrical Engineer, 150 Nassau St., New York City.	May 20, 1890
LUDLAM, HARRY W.	With Western Electric Co., 22 Thames St., New York City.	Dec. 18, 1895
LUNDELL, ROBERT	Electrical Engineer, Interior Conduit and Insulation Co., 527 W. 34th St., New York; residence, 47 Brevoort Pl., Brooklyn, N. Y.	Feb. 7, 1890
LUQUER, THATCHER, T. P.	New York Telephone Co., 18 Cortlandt St., residence, Bedford, N. Y.	June 26, 1891
LYMAN, CHESTER WOLCOTT, *M. A.*	Manager Herkimer Paper, Co., Herkimer, N. Y.	Sept. 19, 1894
LYMAN, JAMES [Life Member.]	839 Union Street, Schenectady, N. Y.	Sept. 19, 1894
MACCOUN, ELLICOTT	Supt. of the Electrical Dep't., The Carnegie Steel Co., Braddock, Pa.	Nov. 20, 1895
MACCULLOCH, ROBERT C.	Manager, Jos. Lough Electric Co., 503 Fifth Ave.; residence, 209 W. 81st St., New York City.	Feb. 27, 1895
MACFADDEN, CARL K.	Electrical Engineer, Gas Engine Dep't Western Gas Construction Co., Fort Wayne, Ind.	Sept. 27, 1892
MACGREGOR, WILLARD H.	With Ward Leonard Electric Co., Hoboken, N. J.; residence, 359 W. 27th St., New York City.	Jan. 20, 1897
MACKIE, C. P.	30 Broad St., New York City; residence, Englewood, N. J.	Mar. 21, 1893
MACKINTOSH, FRED'K.	Electrical Engineer, General Electric Co.; residence, 9 South Church St., Schenectady, N. Y.	Mar. 25, 1896
MACLEOD, GEORGE	Superintendent and Engineer, Kentucky and Indiana Bridge Co., 29th and High Sts., Louisville Ky.; residence, New Albany, Ind.	Aug. 5, 1896
MACMULLAN, ROBERT HEATH	Lafayette, Ind.	Sept. 22, 1891
MADDEN, OSCAR E.	[Address unknown.]	April 15, 1884
MAGEE, LOUIS J.	Electrical Engineer, Director, der Union Elektricitats Gesellschaft, Corneliusstr 1., Berlin, W. Germany.	April 2, 1889
MAKI, HEIICHIR	Chief Engineer, Kioto Traction Co., Suyemarucho Dotemachi Marutamachisagarn, Kioto, Japan.	Aug. 5, 1896
MALIA, JAMES P.	Electrician, Armour & Co., 5316 Union Ave., Chicago, Ill.	June 20, 1894
MANN, FRANCIS P.	[Address unknown.]	June 6, 1893
MANN, ROBERT BRUCE	507 Logan Ave., Milwaukee, Wis.	Sept. 25, 1895
MANSON, JAS. W.	Wire Chief, Franklin Street Exchange. New York Telephone Co.; residence, 973 Asmterdam Ave., New York City.	Mar. 25, 1896
MARTIN, A. J.	Complete Electric Construction Co., 121 Liberty St., New York City.	Mar. 15, 1892

Name.	Address.	Date of Election.
MARTIN, FRANK	Electrical Engineer, Madison Square Garden Company, New York City.	Oct. 21, 1890
MARTIN, JAMES A.	Martin & Witte, Electrical and Mechanical Engineers, 58 Centre St., New York City.	May 19, 1896
MARTIN, T. COMMERFORD	(*Past-President.*) Editor, *The Electrical Engineer*, 203 Broadway, New York City.	April 15, 1884
MASON, JAMES H.	Electrical Expert, 10 Fifth Ave., Brooklyn, N. Y.	May 19, 1891
MATTHEWS, CHARLES P.	Associate Professor, Electrical Engineering, Purdue University, Lafayette, Ind.	May 16, 1893
MAXWELL, EUGENE	Superintendent, Third Street and Suburban Railway Co., Seattle, Wash.	Aug. 5, 1896
MAURO, PHILIP	Counsellor at-Law in Patent Causes (Pollock & Mauro), 620 F. St., Washington, D. C.	Dec. 21, 1892
MAYER, MAXWELL M.	Mfr. of Plating Dynamos, 2d Ave. and 121st St.; residence, 433 East 116th St., New York City.	Feb. 27, 1895
MAYRHOFER, JOS. CARL	Electrical Engineer, 165 W. 82d St., New York City.	June 20, 1894
McBRIDE, JAMES	Superintendent, N. Y. & Boston Dye Wood Co., 146 Kent St., Brooklyn, N. Y.	Sept. 27, 1892
McCARTHY, LAWRENCE A.	Western Union Telegraph Co., New York City, 1053 Bedford Ave., Brooklyn, N. Y.	Jan. 19, 1892
McCARTHY, E. D.	Electrical Engineer, The F. P. Little Electric Construction and Supply Co., 135 Seneca St.; residence, 451 14th Street, Buffalo, N. Y.	Nov. 18, 1896
McCLUER, CHAS. P.	District Inspector, So. Bell Tel. and Tel. Co., Richmond, Va.	Apr. 22, 1896
McCLURG, W. A.	Manager, Electrical Dept., Plainfield Gas and Electric Light Co., 207 Madison Ave., Plainfield, N. J	Dec. 20, 1893
McELROY, JAMES F.	Mechanical Supt., The Consolidated Car Heating Co., 131 Lake Ave., Albany, N. Y.	Nov. 15, 1892
McKAY, C. R.	Consulting Engineer, 140 South Main St., Salt Lake City, Utah.	Dec. 20, 1893
McKIBBIN, GEORGE N.	Reed & McKibbin, General Street Railway Contractors, 30 Broad St., New York City.	June 8, 1887
McKISSICK, A. F.	Professor of Electrical Engineering, The A. & M. College of Ala., Auburn, Ala.	Feb. 16, 1892
McKITTRICK, F. J. A.	Graduate Student Cornell University, 624 Western Avenue, Lynn, Mass.	Aug. 5, 1896
McRAE, AUSTIN LEE	Consulting Electrical Engineer, 306 Oriel Bldg, St. Louis, Mo.	May 17, 1892

Name.	Address.	Date of Election.
MEADOWS, HAROLD GREGORY	Associate Engineer (Elec.) with Newcomb Carlton, 109 White Building; residence, 114 West Chippewa St., Buffalo, N. Y.	Sept. 23, 1896
MEDINA, FRANK P.	Electrician, Pacific Postal Telegraph Co., 534 Market St., San Francisco, Cal.	Sept. 19, 1894
MERCER, ANDREW G.	Electrician, Waterloo Electric Co., Waterloo, N. Y.	Sept. 3, 1889
MEREDITH, WYNN	Electrical Engineer, Hasson & Hunt, 310 Pine St., San Francisco, Cal.	Jan. 17, 1894
MERRILL, E. A.	Electrical Engineer, Pierce & Miller Engineering Co., 26 Cortlandt St., New York City.	Sept. 20, 1893
MERRILL, JOSIAH L.	Ass't to Estimating Engineer of the Construction Department, General Elec. Co., Schenectady, N. Y.	Sept. 25, 1895
MERRITT, ERNEST	Assistant Professor in Physics, Cornell University, Ithaca, N. Y.	Sept. 16, 1890.
MERZ, CHAS. H.	British Thomson-Houston Ltd., 38 Parliament St., London, S.W.; residence, The Quarries, Newcastle-on-Tyne, England.	Sept. 25, 1895
MEYER, JULIUS	Consulting Engineer, 44 Broad St., New York City.	Oct. 25, 1892
MIDDLEMISS, P. R., *M. E.*	Electrical Engineer, General Electric Co., Box 588, Schenectady, N. Y.	Mar. 20, 1895
MILLER, JOSEPH A.	Civil and Consulting Engineer, 25 Butler Exchange, Providence, R. I.	Dec. 9, 1884
MILLER, WM. C., *M. S.*	Electrical Engineer, 3 South Hawk St., Albany, N. Y.	Oct. 21, 1890
MINER, WILLARD M.	Electrician and Inventor, 428 East Sixth St., Plainfield, N. J.	July 12, 1887
MITCHELL, SIDNEY Z.	Manager, Oregon, Washington and Idaho Agency, General Electric Co., Fleischner Building, Portland, Ore.	Nov. 12, 1889
MOORE, WM. E.	General Superintendent, The Augusta Railway & Electric Co., Augusta, Ga.	Jan. 22, 1896
MONELL, JOSEPH T.	Consulting Electrical Engineer, 236 W 22d St., New York City.	Oct. 27, 1891
MONTAGUE, RALPH L.	Chief of Electrical Department, The Gold Dredging Co., Bannack, Mont.	Feb. 26, 1896
MORA, MARIANO LUIS	General Electric Co., Schenectady, N. Y.	Mar. 20, 1895
MORDEY, WM. MORRIS	Electrician, Brush Electrical Engineering Co., Redholm, Loughborough, London, Eng.	Sept. 22, 1891
MORGAN, CHAS. H.	326 St. Paul St., Baltimore, Md.	Aug. 5, 1896
MOREHOUSE, H. H.	General Manager and Electrician, Alumbrado Electrico de Quezaltenango, Apartado, No 44, Quezaltenango, Guatemala, C. A.	Feb. 21, 1894

Name.	Address.	Date of Election.
MORLEY, EDGAR L.	Sup't Hatzel & Buehler, 114 5th Ave., New York City.	Sept. 25, 1895
MORRISON, J. FRANK	15 South St., Baltimore, Md.	April 15, 1884
MORSE, GEORGE H.	Wagner Electric Mfg. Co. St. Louis, Mo.	May 15, 1894
MORSS, EVERETT	Vice-President, Simplex Electric Co., 303 Marlboro St., Boston, Mass	Sept. 22, 1891
MORTON, HENRY, *Ph.D.*	President of Stevens Institute of Technology, Hoboken, N. J.	May 24, 1887
MOSES, DR. OTTO A.	Electrician, 1037 Fifth Ave., New York City.	May 17, 1887
MOSES, PERCIVAL ROBERT, *E. E.*	Consulting Electrical Engineer, 120 Broadway : residence, 46 West 97th St., New York Ci.y.	Dec. 19, 1894
MOSMAN, CHAS. T.	Power and Mining Engineering Dep't., General Electric Co.; residence, 406 Union St., Schenectady, N. Y.	Mar. 20, 1895
MOSSCROP, WM. A., *M.E*	Electrical Engineer, care R. W. Pope, Sec'y, 26 Cortlandt St., N. Y. City.	May 7, 1889
MOTT, S. D.	Electrical Engineer and Inventor, Passaic, N. J.	Sept. 20, 1893
MUNNS, CHAS. K.	Electrician, Strowger Autom. Tel. Exchange, 947 Rookery, Chicago, Ill.	Nov. 21, 1894
MUSTIN, HERBERT S.	[Address unknown.]	Dec. 20, 1893
MYERS, L. E.	Secretary and Treasurer, Electrical Installation Co., 917 Monadnock Building, Chicago, Ill.	Sept. 19, 1894
NEWBURY, F. J.	Manager Insulated Wire Department, John A. Roebling's Sons Co., Trenton, N. J.	Sept. 23, 1896
NICHOLSON, WALTER W.	General Supt. Central N. Y. Telephone and Telegraph Co., 73 Howard Ave., Utica, N. Y.	May 15, 1894
NOCK, GEO. W.	Chief Engineer, in charge of Steam and Electric Plant Westinghouse Elect. and Mfg. Co., Pittsburg, Pa.	Aug. 5, 1896
NORTON, ELBERT F.	With Card Electric Motor and Dynamo Co., 622-3 Western Union Building, Chicago, Ill.	Dec. 20, 1893
NOXON, C. PER LEE	Contracting Electrical Engineer, 504 Townsend St., Syracuse, N. Y.	Oct. 17, 1894
NUNN, RICHARD J., *M.D.*	Physician, 119½ York St., Savannah, Ga.	July 12, 1887
NYHAN, J. T.	Superintendent and Electrician, Macon and Indian Spring Electric Railway, Macon, Ga.	Feb. 27, 1895
OCKERSHAUSEN, H. A.	Electrical Engineer, 65 Madison Ave., Jersey City, N. J.	Sept. 6, 1887
OLAN, THEODOR, J. W.	Civil and Electrical Engineer, 68 West 49th St., New York City.	May 16, 1893
OLIVETTI, CAMILLO	Ingegnere Industriale, Ivrea, Italy.	Oct. 17, 1894

Name.	Address.	Date of Election.
ORMSBEE, ALEX. F.	Electrical Engineer; residence, 183 Joralemon St., Brooklyn, N. Y.	June 27, 1895
OSBORNE, LOYALL ALLEN	Assistant to 2nd Vice-President Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	Oct. 18, 1893
OSTERBERG, MAX, *E.E.*, *A.M.*	Consulting Engineer, and Electrical Expert, 27 Thames St.; residence, 113 E. 65th St., New York City.	Jan. 17, 1894
O'SULLIVAN, M. J.	Superintendent, Electric Light, B. & O. R. R. Co., 154 Keen Street, Zanesville, O.	Mar. 20, 1895
OTTEN, DR. JAN D.	Engineer, Union Elektricitats Gesellschaft, Kurfürstenstrasse 97 III, Berlin, W. Germany.	Nov. 18, 1890
OWENS, R. B.	Professor of Electrical Engineering, University of Nebraska, Lincoln, Neb.	June 17, 1890
PAGE, A. D.	Assistant Manager, General Electric Co. Lamp Works, Harrison, N. J.	Jan. 19, 1892
PARCELLE, ALBERT L.	Electrician and Inventor, 157 Washington St., Boston, Mass.	Dec. 16, 1891
PARKER, HERSCHEL C.	Tutor in Physics, Columbia University, 21 Fort Green Pl., Brooklyn, N. Y.	April 19, 1892
PARMLY C. HOWARD, *S.M.*, *E.E.*	College of the City of New York, 17 Lexington Ave.; residence, 344 W. 29th St., New York City.	Feb. 21, 1893
PARRY, EVAN	Engineer, The British Thomson-Houston Ltd., Sunningdale. Fitzgerald Ave., Barnes, London, Eng.	Sept. 25, 1895
PARSELL, HENRY V., JR.	31 E. 21st St., New York City.	Nov. 12, 1889
PATTON, PRICE I.	Sheble & Patton, Ltd., 1026 Filbert St.; residence, 3926 Walnut St., Philadelphia, Pa.	Mar. 20, 1895
PECK, EDWARD F.	15 Cortlandt Street, New York City; residence, 87 Monroe St., Brooklyn, N. Y.	May 20, 1890
PEDERSEN, FREDERICK MALLING	Assistant Electrical Engineer, Crocker-Wheeler Electric Co., Ampere, Newark, N. J.; residence, 118 W. 104th St., New York City.	Sept. 20, 1893
PEIRCE, ARTHUR W. K.	Simmer and Jack Gold Mining Co., Johannesburg, S. A. R.	June 27, 1895
PEIRCE, WM. H.	Assistant Manager, Baltimore Smelting and Rolling Co., Keyser Bldg, German and Calvert Sts., Baltimore, Md.	Sept. 7, 1888
PERKINS, FRANK C.	Electrical Engineer and Contractor, 774 Prospect Ave., Buffalo, N. Y.	Oct. 21, 1890
PETTY, WALTER M.	Superintendent Fire Alarm Telegraph, Rutherford, N. J.	May 16, 1893
PFUND, RICHARD	With Western Union Telegraph Co., 195 Broadway, New York City.	April 18, 1893

Name.	Address.	Date of Election.
PHELPS, WM. J.	Electrical Engineer and Contractor, Monadnock Bldg.. Chicago; residence, Elmwood, Ill.	Mar. 25, 1896
PHILBRICK, B. W.	Electrician, in charge of Electrical Plant, Hon. Levi P. Morton, Rhinecliff, N. Y.	May 15, 1894
PHILLIPS, EUGENE F.	President, American Electrical Works, Phillipsdale, Providence, R. I.	July 13, 1889
PHILLIPS, LEO A.	Westinghouse Electric and Mfg. Co., East Pittsburg, Pa.	Mar. 21, 1894
PHISTERER, FRED'K WILLIAM	107 Columbia St., Albany, N. Y.	Nov. 20, 1895
PINKERTON, ANDREW	Electrical Engineer, The Apollo Iron and Steel Co., Apollo, Pa.	Sept. 25, 1895
PLUMB, CHARLES	Proprietor and Electrician, The Chas. Plumb Electrical Works, 70 West Swan St., Buffalo, N. Y.	June 20, 1894
POOLE, CECIL P.	58 New Street; residence, 206 W. 80th St., New York City.	Jan. 3, 1888
POPE, RALPH WAINWRIGHT	Secretary to the American Institute of Electrical Engineers, 26 Cortlandt St., (Telephone, 2199 Cortlandt), New York City; residence, 570 Cherry St., Elizabeth, N. J.	June 2, 1885
PORTER, H. HOBART, JR.	Agent, Westinghouse Elec. and Mfg. Co., 120 Broadway, New York; residence, Lawrence, L. I.	Mar. 25, 1896
POTTER, HENRY NOEL	Electrician, Steglitzer Strasse, 10 parterre, Berlin W., Germany.	Sept. 19, 1894
POWELL, PERCY HOWARD	Construction Dep't. New York Telephone Co., 18 Cortlandt St., New York City; residence, Hempstead, N. Y.	Sept. 25, 1895
PRICE, CHAS. W.	Editor the *Electrical Review*, Times Building, New York City; residence, 223 Garfield Place, Brooklyn, N. Y.	Sept 19, 1894
PRICE, EDGAR F.	Electricial Engineer, Carbide Works, Niagara Falls, N. Y.	June 27, 1895
PRINCE, J. LLOYD	868 Flatbush Ave., (Flatbush Station), Brooklyn, N. Y.	Feb. 27, 1895
PRIVAT, LOUIS	Electrician, Cicero Water, Gas and Electric Light Co., Oak Park, Ill.	Dec. 19, 1894
PROCTOR, THOS. L.	General Manager, Riker Electric Motor Co., Brooklyn; residence, Newtown, L. I., N. Y.	April 18, 1894
PUPIN, DR. MICHAEL I.	(*Vice-President*) Adjunct Professor in Mechanics, Columbia University; residence, 7 Highland Place, Yonkers N. Y.	Mar. 18, 1890
RANDALL, JOHN E.	Columbia Incandescent Lamp Co., 1912 Olive St., St. Louis, Mo.	May 7, 1889
RANDOLPH, L. S.	Professor of Mechanical Engineering, Blacksburg, Va.	Feb. 21, 1893

Name.	Address.	Date of Election.
RATHENAU, ERICH	Electrical Engineer, Allg. Electricitats Gesellschaft, Berlin, Germany.	Nov. 20, 1895
RAY, WILLIAM D.	General Manager Everett Railway and Electric Co., Everett, Washington.	Sept. 27, 1892
READ, ROBERT H.	Patent Attorney, 44 Broad St., New York City.	Jan. 19, 1892
REDMAN, GEO. A.	General Supt., Electric Dept., Brush Elec. Light Co., and Rochester Gas and Elec. Co., Rochester, N. Y.	Feb. 27, 1895
REED, CHAS. J.	Electrician, 3313 N. 16th St., Philadelphia, Pa.	Mar. 5, 1889
REED, HARRY D.	Electrician, Bishop Gutta Percha Co., 420 East 25th St., New York City; residence, 88 North 9th St., Newark, N. J.	Sept. 19, 1894
REED, HENRY A.	Secretary and Manager, Bishop Gutta-Percha Co., 422 East 25th St, New York City; residence, 88 North 9th St., Newark, N. J.	June 4, 1889
REID, EDWIN S.	General Sup't of Construction, National Underground Cable Co, 17 Times Building, New York City; residence, 116 W. 11th St.	Feb. 26, 1896
REID, THORBURN	Electrical Engineer, care British Thomson-Houston Co., 18 Parliament St., London, S. W., Eng.	Oct. 21, 1890
REILLY, JOHN C.	General Supt., N. Y. & N. J. Tel. Co., 16 Smith St., Brooklyn, N. Y.	April 15, 1884
RENNARD, JOHN CLIFFORD,	*A. B. E. E.* Consulting and Supervising Electrical Engineer, 18 Cortlandt St.; residence, 302 W. 73d St., New York City.	Jan. 16, 1895
REQUIER, A. MARCEL	Electrical Engineer, Westinghouse Electric and Manufacturing Co., Pittsburg, Pa.	Dec. 20, 1893
RHODES, S. ARTHUR	Electrician, Chief Testing Department, Chicago Telephone Co, Chicago, Ill.; residence, 429 North Pine Ave., Austin, Ill.	Oct. 17, 1894
RICE, ARTHUR L.	Professor of Steam and Electrical Engineering, Pratt Institute, Brooklyn, N. Y.	Oct. 21, 1896
RICE, CALVIN WINSOR	Consulting Electrical Engineer, 8 Eaton St., Winchester, Mass.	Jan. 20, 1897
RICHARDS, CHAS. W.	Partner, Cumner-Richards Co., 69 Broad Street, Boston; residence, Needham, Mass.	Sept. 23, 1896
RICHARDSON, ROBERT E.	Electrical Engineer, Pierce & Richardson, 1409 Manhattan Building; residence, 3622 Michigan Ave, Chicago, Ill.	Sept. 19, 1894
RICKER, CHARLES W.	Expert Electrical Engineer, 184 Cleveland Ave., Buffalo, N. Y.	May 15, 1894

Name.	Address.	Date of Election.
RIDEOUT, ALEXANDER C.	*LL. D.*, Consulting Electrical and Mechanical Engineer, 101 Randolph St., Chicago, Ill.	Aug. 5, 1896
RIDLEY, A. E. BROOKE	Agent, Electrical Engineer, Siemens & Halske Electric Co., 10 Front St., San Francisco, Cal.	Nov. 21, 1894
RITTENHOUSE, CHAS. T.	Editor the *Electrical World*, 253 Broadway; residence, 247 W. 138th St, New York City.	Feb. 21, 1894
ROBERSON, OLIVER R.	Electrician, Western Union Telegraph Co., 195 Broadway, P. O. Box 856, New York City.	Dec. 20, 1893
ROBERTS, WM. H.	413 Harrison St., Cincinnati, O.	Sept. 19, 1894
ROBINSON, ALMON	Draughtsman, Expert in Methods of Gearing, Webster Road, P. O. Box 943, Lewiston, Me.	Sept. 6, 1887
ROBINSON, DWIGHT PARKER	With Stone & Webster, 4 P. O. Square, Boston, Mass.	Sept. 25, 1895
ROBINSON, FRANCIS G.	With Brooklyn Heights R. R. Co.; residence, 156 Macon St., Brooklyn, N. Y.	Nov. 21, 1894
RODMAN, SAMUEL, JR.	(Late 1st Lieut., 2nd U. S. Artillery), Electrician and Expert in High Explosives, Room 106, Pullman, Bldg., Chicago, Ill.	Sept. 16, 1890
ROEBLING, FERDINAND W.	Manufacturer of Electrical Wires and Cables, Trenton, N. J.	June 8, 1887
ROESSLER, S. W.	Captain, Corps of Engineers U. S. A., Willets Point, N. Y.	Dec. 3, 1889
ROLLER, FRANK W. *M. E.*	Electrical Engineer, Machado & Roller, Electrical Machinery, 203 Broadway, N. Y.; residence, Cranford, N. J.	May 21, 1895
ROPER, DENNEY W.	Edison Illuminating Co. of St. Louis, Mo., Alton, Ill.	June 6, 1893
ROSA, EDWARD B.	Professor of Physics, Wesleyan University, Middletown, Conn.	Feb. 17, 1897
ROSEBRUGH, THOMAS REEVE	Lecturer in Electrical Engineering, School of Practical Science, Toronto, Ont.	June 26, 1891
ROSENBAUM, WM. A.	Electrical Expert and Patent Solicitor, 177 Times Building, New York City.	Jan. 3, 1889
ROSENBERG, E. M., *M. E.*	Residence, 138 W. 85th St., New York City.	Oct. 21, 1890
ROSS, TAYLOR WILLIAM	Second Assistant Engineer, U. S. Revenue Cutter Service, Revenue Cutter "McLane," Key West, Fla.	Mar. 25, 1896
ROWLAND, ARTHUR JOHN	Professor of Electrical Engineering, Drexel Institute; residence, 3220 Spencer Terrace, Philadelphia, Pa.	Sept. 19, 1894
ROWLAND, HENRY A.	Professor of Physics, Johns Hopkins University, Baltimore, Md.	Mar. 21, 1894
ROYCE, FRED W.	Electrician and Patent Solicitor, 1410 Pennsylvania Ave., Washington, D. C.	April 15, 1884

Name.	Address.	Date of Election.
RUSHMORE, DAVID B.	Foreman, Testing Dep't Royal Electric Co., Montreal, P. Q.	Sept. 25, 1895
RUTHERFORD, W. M.	Chief Engineer, Canadian General Electric Co., 65 Front St., W. Toronto, Can.	Sept. 22, 1891
SACHS, JOSEPH	Devising and Consulting Electrical Engineer, 32 Nassau St., New York City.	Mar. 15, 1892
SACKETT, WARD M.	Assistant Chief Draughtsman, Chicago Telephone Co., residence 3739 Ellis Ave., Chicago, Ill.	Oct. 17, 1894
SAGE, HENRY JUDSON	Sage & Co., Electrical Engineers, Rochester, Pa.	Dec. 20, 1893
SAHULKA, DR. JOHANN	Docent of Electrotechnics, Technische Hochschule, Vienna, Austria	Dec. 20, 1893
SAMPSON, F. D.	Manager, Charlotte Electric Light and Power Co., Charlotte, N. C.	Aug. 5, 1896
SANBORN, FRANCIS N.	Torrington, Conn.	Nov. 24, 1891
SANDERSON, EDWIN N.	Of Sanderson & Porter, Engineers and Contractors, 120 Broadway, New York City.	Oct. 17, 1894
SARGENT, HOWARD R.	Electrical Engineer, General Electric Co.; residence, 242 Union Street, Schenectady, N. Y.	Mar. 25, 1896
SATHERBERG, CARL HUGO	Chief Engineer, The Midvale Steel Co., Nicetown, Phila., Pa.; residence 1752 N. 26th St., Philadelphia, Pa.	Aug. 5, 1896
SAWYER, FRED. W.	68 Mount Vernon St., Fitchburg, Mass.	June 27, 1895
SAXELBY, FREDERICK	Electrical Engineer, 288 Summer Ave., Newark, N. J.	June 5, 1888
SCHEIBLE, ALBERT	Manager for George Cutter, 851 The Rookery, Chicago, Ill.	June 20, 1894
SCHLOSSER, FRED. G.	Superintendent of Electric Dept., Laclede Gas Light Co., 411 N. 11th St. Louis, Mo.	Sept. 22, 1891
SCHREITER, HEINR. *C. E.*	Counsellor and Attorney, 20 Nassau St., New York City.	Jan. 17, 1893
SCHWAB, MARTIN C.	1729 Madison Ave., Baltimore, Md.	Nov. 18, 1896
SCHWABE, WALTER P.	Electrician, Rutherford, Boiling Springs and Carlstadt Electric Co., Carlstadt, N. J.	May 19, 1896
SCIDMORE, FRANK L.	With N. Y. C. & H. R. R. R. Co., office of A. F. A.; residence, 106 Hawthorne Ave., Yonkers, N. Y.	Dec. 18, 1895
SCOTT, JAMES B.	Eleetrical and Mechanical Englneer, 227 East German St., Baltimore Md.	Aug. 5, 1896
SEARING, LEWIS	Consulting, Mechanical, and Electrical Engineer, Denver Engineering Works, Denver, Col.	April 3, 1888
SEARLES, A. L.	Engineering Dept., Fort Wayne Electric Corporation, Fort Wayne, Ind.	April 18, 1894

Name.	Address.	Date of Election
SEE, A. B.	A. B. See Manufacturing Co., 116 Front St.; residence, 107 East 19th St., (Flatbush), Brooklyn, N. Y.	Jan. 17, 1893
SEELY, J. A.	Electrical Engineer and Contractor, 121 Liberty St., New York City.	April 15, 1884
SEITZINGER, HARRY M.	Consulting and Constructing Engineer, 6 Northampton St., Wilkes-Barre, Pa.	Sept. 20, 1893
SERRELL, LEMUEL WM.	Mechanical and Electrical Engineer, 99 Cedar St., New York City; residence, Plainfield, N. J.	Nov. 1, 1887
SERVA, A. A	With Fort Wayne Electric Corporation, 17 Federal St., Boston, Mass.	Dec. 20, 1893
SHAIN, CHARLES D.	136 Liberty St., New York City.	June 7, 1892
SHARP, CLAYTON H.	Instructor, Department of Physics, Cornell University, 122 University Ave , Ithaca, N. Y.	May 15, 1894
SHARPE, E. C.	Consulting Electrical Engineer, 524 S. Broadway, Los Angeles, Cal.	Feb. 26, 1896
SHEDD, JOHN C.	Professor of Physics and Applied Electricity, Marietta College; residence, 512 Fourth St., Marietta, Ohio.	Dec. 19, 1894
SHEEHY, ROBERT J.	President, Sheehy Automatic Railroad Signal Co., 122 Pearl St., Boston, Mass.	April 21, 1891
SHIELDS, W. J.	Consulting Engineer, New Wilmington, Pa.	Sept. 19, 1894
SHOCK, THOS. A. W.	Gen'l Sup't Sacramento Electric Power and Light Co., Sacramento, Cal.	Mar. 20, 1895
SHONNARD, HAROLD W.	[Address unknown.]	Oct. 23, 1895
SIMPSON, ALEXANDER B.	Estimating Engineer, Western Electric Co., N. Y. City ; residence, 125 2nd Place, Brooklyn, N. Y.	May 21, 1895
SISE, CHARLES F.	President, Bell Telephone Co., of Canada, P. O. Box 1918, Montreal, Canada.	June 8, 1887
SKIRROW, JOHN F.	Ass't Manager, Postal Telegraph Cable Co., New York City; residence, 183 N. 19th St , East Orange, N. J.	Sept. 25, 1895
SLADE, ARTHUR J., *Ph.D.*	Engineer, with George Hill, 44 Broadway; residence, 62 East 66th St., New York City.	Sept. 19, 1894
SLATER, FREDERICK R.	Designing Department, Otis Bros. & Co., 153 Warburton Ave., Yonkers, N. Y.	Oct. 17, 1894
SMITH, CHARLES HENRY, JR.	Box 2, Atlanta, Ga.	Jan. 17, 1894
SMITH, FRANK E.	Chief Electrician, Edison Light and Power Co., 229 Stevenson St., San Francisco, Cal.	Sept 19, 1894

Name.	Address.	Date of Election.
SMITH, FREDERICK H.	Civil Engineer, 216 Equitable Bldg., Baltimore. Md.	Nov. 12, 1889
SMITH, HAROLD BABBITT	Professor of Electrical Engineering, Worcester Polytechnic Institute, Worcester, Mass.	Nov. 24, 1891
SMITH, J. BRODIE	Supt. and Electrician. Manchester Electric Light Co., 142 Merrimack St., Manchester, N. H.	Mar. 21, 1894
SMITH, J. ELLIOT	Superintendent Fire Alarm Telegraph, 122 W. 73d St., New York City.	April 15, 1884
SMITH, OBERLIN	President and Mechanical Engineer, Ferracute Machine Co., Lochwold, Bridgeton, N. J.	May 19, 1891
SMITH, T. JARRARD	Manager Electrical Dept., The E. S. Greeley & Co., 7 Dey St., New York City.	April 19, 1892
SPEED, BUCKNER	Assistant Electrical Engineer, Louisville Electric Light Co., 1521 4th Street, Louisville, Ky.	Apr. 22, 1896
SPENCER, THEODORE	With Bell Telephone Co., 406 Market St., Philadelphia, Pa.	Mar. 21, 1893
SPROUT, SIDNEY S.	Electrical Engineer, 328 Montgomery St., San Francisco, Cal.	Jan. 17, 1894
SQUIER, GEORGE O., *Ph.D.*	1st Lieut., 3d Artillery, Fortress Monroe, Va.	May 19, 1891
STADELMAN, WM. A.	Agent, Elwell-Parker Co., 26 Cortlandt St., New York City.	Feb. 7, 1890
STAHL, TH.	Creusot Works, Creusot, France.	Nov. 15, 1892
STAKES, D. FRANKLIN	Electrical Expert and Salesman, The Fort Wayne Electric Corporation, 101 The Bourse, Philadelphia, Pa.	Jan. 20, 1897
STANLEY, WILLIAM	Electrician, Pittsfield, Mass.	Dec. 6, 1887
STANTON, CHAS. H.	With C. H. & H. Stanton Electrical Contractors, 1517 Walnut St.; residence, 134 S. 3d St., Philadelphia, Pa.	Mar. 20, 1895
STEVENS, J. FRANKLIN	Manager, Keystone Electrical Instrument Co., 9th St. and Montgomery Ave.; residence, 1419 Walnut St., Philadelphia, Pa.	Sept. 19, 1894
STEWART, ROBERT STUART	Supt. of Lines, Public Lighting Commission, 440 Jefferson Ave., Detroit, Michigan.	Dec. 20, 1896
STEWART, W. M.	Wire Chief, New York Telephone Co., 18 Cortlandt Street, New York City; residence, 301 W. 46th St., Flat 14, New York City.	Mar. 25, 1896
STINE, WILBUR M.	(*Vice-President.*) Director Electrical Dept., Armour Institute; residence, 635 W. 61st Street, Chicago, Ill.	May 15, 1894
STOCKBRIDGE, GEO. H.	Patent Attorney, 95 Nassau Street; residence, 2514 11th Ave, near 187th St., New York City.	May 24, 1887

Name.	Address.	Date of Election.
STONE, CHARLES A.	With Firm of Stone & Webster, 4 P. O. Sq., Boston, Mass.	May 19, 1891
STONE, JOSEPH P.	Electrical Engineer, General Electric Co.; residence, 213 Liberty Street, Schenectady, N. Y.	Dec. 18, 1895
STORER, NORMAN W.	Electrical Engineer, Westinghouse Electric and Mfg. Co., Pittsburg; residence, Wilkinsburg, Pa.	Dec. 18, 1895
STORRS, PROF. H. A.	Professor of Electrical Engineering, University of Vt., Burlington, Vt.	Mar. 21, 1893
STRATTON, ALEX.	Assistant Electrical Engineer, C. & C. Electric Co., Garwood, N. J.; residence 21 East 130th St., New York City.	Mar. 20, 1895
STRAUS, THEODORE	Electrical Eng., General Electric Co., Schenectady, N. Y.; residence, 1213 Linden Avenue, Baltimore, Md.	Nov. 18, 1896
STRAUSS, HERMAN A.	Consulting Electrical and Mechanical Engineer, 54 Maiden Lane; residence, 17 W. 69th St., New York.	Oct. 17, 1894
STRONG, FREDERICK G.	734 Equitable Building, Atlanta, Ga.	Oct. 27, 1891
STURTEVANT, CHARLES L.	Patent Attorney, Atlantic Building, Washington, D. C.	Dec. 20, 1893
SULLIVAN, EDWARD	Supt. Construction, Standard Underground Cable Co., 18 Times Bldg.; residence, 337 W. 18th Street, New York City.	Feb. 26, 1896
SUMMERS, LELAND L.	Electrical Engineer, 441 The Rookery, Chicago, Ill.	Feb. 16, 1892
SVENTORZETZKY, CAPT. LOUDOMIR	Military Engineering Academy, St. Petersburg, Russia.	Sept. 20, 1893
SWENSON, BERNARD VICTOR	Assistant Professor of Electrical Engineering, University of Illinois, Champaign, Ill.	Feb. 27, 1895
SWEET, HENRY N.	Chief of Patent Bureau, Thomson Electric Welding Co., 4 Spruce St., Boston, Mass.	May 20, 1890
SYKES, HENRY H.	Chief Engineer, Bell Telephone Co., of Mo., Telephone Bldg., St. Louis, Mo.	Oct. 18, 1893
TAIT, FRANK M.	Superintendent, Catasauqua Electric Light and Power Co., 731 3d St., Catasauqua, Pa.	Sept. 19, 1894
TAPLEY, WALTER H.	Electrician in Government Printing Office, care of Public Printer, Washington, D. C.	Oct. 25, 1892
TEMPLE, WILLIAM CHASE	Mechanical and Electrical Engineer, Lewis Block, P. O. Box 800, Pittsburg, Pa.	May 3, 1887
TESLA, NIKOLA	Electrical Engineer and Inventor, 46 E. Houston St., The Gerlach, 53 W. 27th St., New York City.	June 5, 1888

Name.	Address.	Date of Election.
THAYER, GEORGE LANGSTAFF	Manager, Belle Plaine Electric Light Co., Belle Plaine, Ia.	Aug. 5, 1896
THOMAS, ROBERT MCKEAN, *E. E.*	Assistant Chief Inspector, Bureau of Electrical Appliances, N. Y. Fire Dept.; residence, 135 Madison Ave., New York City.	April 22, 1896
THOMPSON, WILLIAM GEO. MACNEILL	Resident Engineer, Sault Ste. Marie Canal, St. Catharines, Ont.	July 12, 1887
THORDARSSON, CHESTER H.	Chicago Edison Co.; residence, 284 Rush St., Chicago, Ill.	Dec. 18, 1895
THRESHER, ALFRED A.	Electrical Engineer and Proprietor Thresher Electric Co., Dayton, O.	April 22, 1896
THURBER, HOWARD F.	General Superintendent, New York Telephone Co., 18 Cortlandt Street, New York City; residence, 49 Sidney Place, Brooklyn, N. Y.	Mar. 25, 1896
TOERRING, C., JR.	Electrical Engineer, Helios Electric Co., 3214 Arlington Avenue., Philadelphia, Pa.	April 18, 1894
TORCHIO, PHILIPPO	Engineering Dep't, The Edison Elec. Illuminating Co., 53 Duane Street, New York City.	June 27, 1895
TOWER, GEORGE A.	Electrical Engineer, The Sherwood Land Co., and The Jefferson Hotel Co., 109 S. First St., Richmond, Va.	May 15, 1894
TOWNSEND, HENRY C.	Attorney and Expert in Electrical Cases, 5 Beekman St., New York City.	July 10, 1888
TOWNSEND, SAMUEL G. F.	Electrical Engineer in Testing Department, with Ward Leonard Electric Co., Hoboken, N. J.; residence, 131 Fifth Ave., New York City.	Jan. 20, 1898
TREADWELL, AUGUSTUS, JR.	Private Assistant, Polytechnic Institute, 488 3d St., Brooklyn, N. Y.	Feb. 21, 1894
TROTT, A. H. HARDY [Life Member.]	Beer, near Axminster, Devonshire, Eng.	Jan. 20, 1891
TUTTLE, GEORGE W.	Electrical Engineer. Sawyer-Man Electric Co., 510 W. 23d St.; residence, 328 W. 23d St., New York City.	Mar. 17, 1891
VAIL, THEO. N.	26 Cortlandt St., New York City.	April 15, 1884
VAN BUREN, GURDON C.	Electrician and Electrical Contractor, 84 Clinton Ave., Albany, N. Y.	Oct. 25, 1892
VANDEGRIFT, JAMES A.	Westinghouse Electric and Mfg. Co., residence, 158 Ridge Ave., Allegheny, Pa.	Nov. 24, 1891
VANDERSLICE, G. HAMILTON	326 Penn Avenue. Pittsburg, Pa.	Dec. 19, 1894
VAN DEVENTER, CHRISTOPHER	Student, Columbia University; residence, 626 Lexington Ave., New York City.	Feb. 17, 1897

Name.	Address.	Date of Election.
VAN VLECK, FRANK	President, Van Vleck Tramway Co., Wells Fargo Bldg., Los Angeles, Cal.	Nov. 16, 1886
VAN VLECK, JOHN FALCONER	Constructing Engineer, The Edison Electric and Illuminating Co. of New York; residence, Glenridge, N. J.	Aug. 5, 1896
VAN WYCK, PHILIP V. R., JR.	Plainfield, N. J.	April 21, 1891
VARLEY, THOMAS W.	Electrician, United Electric Light and Power Co., 210 Elizabeth St., New York City.	Sept. 19, 1894
VARNEY, WILLIAM WESLEY	Attorney at Law, Electrical Expert, 118 East Lexington St.; residence, 1001 Harlem Ave., Baltimore, Md.	Nov. 21, 1894
VERLEY, HORACE S. L.	With Dr. Wm. E. Geyer, as Laboratory Assistant, Stevens Institute, Hoboken, N. J.; 136 Liberty St., New York City.	May 17, 1892
VOIT, DR. ERNST	Professor of Electricity, Technical University, Schwanthalerstrasse, Munchen, Germany.	Mar. 21, 1894
VOSMAER, ALEXANDER	Mechanical, Chemical and Electrical Engineer, The General Ozone and Electric Supply Co., Suerkade 104, The Hague, Holland.	Nov. 18, 1896
WACKER, GEORGE G.	1340 Vanderbilt Ave., New York City.	Sept. 6, 1887
WAGNER, EDWARD ANDREWS.	Electrician, The Mexican International R. R. Co., Eagle Pass, Texas	Jan. 22, 1896
WALKER, ARTHUR F.	Sup't and Electrical Engineer, Edison Light Co., Grand Rapids, Mich.	Oct. 23, 1895
WALLACE, CHAS. F.	Engineer, Stone and Webster, Boston, Mass.; residence, 62 Forest Street, Roxbury, Boston, Mass.	Nov. 18, 1896
WALLACE, GEO. S.	Telegraph Office Manager, Chesapeake & Ohio Ry. Co., Box 214, Huntington, W. Va.	Oct. 25, 1892
WALLACE, WILLIAM	Wire Manufacturer, Ansonia, Conn.	April 15, 1884
WARDELL, GEORGE PHELPS	Secretary, Department of Science and Technology, Pratt Institute, Brooklyn, N. Y.	Nov. 12, 1889
WARDLAW, GEORGE A.	112 East Willow St., Syracuse, N. Y.	Jan. 17, 1894
WARING, RICHARD S.	Standard Underground Cable Co., 61 Westinghouse Bldg., Pittsburg, Pa.	April 15, 1884
WARNER, CHAS. H.	Consulting Electrical Engineer, 50 Broadway, New York City.	Dec. 20, 1893
WARREN, ALFRED K.	Proprietor, A. K. Warren & Co., 451 Greenwich St., New York; residence New Brighton, S. I., N. Y.	Nov. 20, 1895
WASON, CHAS. W.	Electrical Engineer and Purchasing Agent, Cleveland Electric R. R. Co., 2069 Euclid Ave., Cleveland, O.	May 19, 1891

Name.	Address.	Date of Election.
WASON, LEONARD C.	Vice-Prest., The Aberthaw Co., 31 State Street, Boston ; residence, 199 Harvard St., Brookline, Mass.	Dec. 20, 1893
WATERS, EDWARD G.	Resident Manager, General Electric Co., 308 Times Bldg., Pittsburg, Pa.	Mar. 18, 1890
WATSON, ROBERT	Patent Attorney, 931 F. St., N. W., Washington, D. C.	Oct. 21, 1890
WATTS, H. FRANKLIN	Electrical Engineer and Contractor, 5171 Heston St., Philadelphia, Pa.	May 20, 1890
WEAVER, NORMAN R.	Box 87, Selma, Ala.	Oct. 25, 1892
WEBB, HENRY STORRS	Instructor in Electrical Engineering, Lehigh University, South Bethlehem, Pa.	Nov. 20, 1895
WEBSTER, DR. ARTHUR G.	Assistant Professor of Physics, Clark University, 936 Main St., Worcester, Mass.	Jan. 19, 1892
WEBSTER, EDWIN S.	Firm of Stone & Webster, 4 P. O. Sq., Boston, Mass.	April 21, 1891
WENDLE, GEORGE E.	760 W. 4th St., Williamsport, Pa.	Feb. 21, 1894
WEST, JULIUS HENRIK	Engineer, Handjery St., 58 Friedenau, Berlin, Germany.	Sept. 20, 1893
WELLES, FRANCIS R.	Manufacturer, 46 Avenue de Breteuil, Paris, France.	Sept. 6, 1887
WHARTON, HUGH M.	Electrical Engineer, 69 Christopher St., Montclair, N. J.	May 15, 1894
WHITAKER, S. EDGAR	Electrical Engineer and Contractor, 58 Oliver St., Fitchburg, Mass.; residence, 93 High Rock Avenue, Lynn, Mass.	Aug. 5, 1896
WHITE, CHAS. G.	Public Schools Sup't, and Instructor in Physics and Chemistry, Lake Linden, Mich.	Sept. 23, 1896
WHITE, J. G.	J. G. White & Co., Electrical Engineers and Contractors, 29 Broadway, New York City.	April 2, 1889
WHITE, WILL F.	Electrical Engineer, Vice-President, New Omaha T.-H. Electric Light Co., 309 So. 13th St., Omaha, Neb.	Feb. 7, 1890
WHITING, ALLEN H.	Electrical Engineer, Riker Electric Motor Co., Brooklyn, N. Y.; residence, Stamford, Conn.	Nov. 18, 1896
WHITMORE, W. G.	Electrical Engineer, General Electric Co., Edison Building, Box 3067, New York City.	Mar. 18, 1890
WHITNEY, HENRY M. [Life Member.]	81 Milk St., Boston, Mass.	July 12, 1887
WIESE, GUSTAV ADOLPH	City Electrician of Alameda, 718 Haight Ave., Alameda, Cal.	Sept. 25, 1895
WIGHTMAN, MERLE J.	Electrical Engineer, The Staten Island Midland Railway Co., Stapleton, N. Y.	Mar. 5, 1889

Name.	Address.	Date of Election.
WILEY, WALTER S.	Engineer, with the American Waterworks, 1107 No. 40th St.. Omaha, Neb.	April 18, 1894
WILEY, WM. H.	Scientific Expert, 53 E. 10th St., New York City.	Feb. 7, 1888
WILLIAMS, CHARLES JR.	Electrician, 1 Arlington Street, East Somerville, Mass.	April 15, 1884
WILLIAMSON, G. DEWITT	Dobbs Ferry, N. Y.	April 18, 1893
WILSON, CHESTER P.	Chief Engineer, East St. Louis Plant, Swift & Co., 31 Nicholson Place, St. Louis, Mo.	Sep. 25, 1895
WINAND, PAUL A. N.	Engineer and Supt., Schleicher, Schumm & Co., 3200 Arch St., Philadelphia, Pa.	June 20, 1894
WINCHESTER, SAMUEL B.	9 Laurel St., Holyoke, Mass.	May 15, 1894
WINSLOW, I. E.	The General Traction Company, Ltd., 35 Parliament Street, Westminster, London, Eng.	Nov. 12, 1889
WINTRINGHAM, J. P.	Theorist, 36 Pine St., New York City, and 153 Henry St., Brooklyn, N.Y.	May 7, 1889
WIRT, HERBERT C.	Engineer, Supply Department, General Electric Co., Schenectady, N. Y.	June 26, 1891
WOODWARD, FRANCKE L.	Electrical Engineer, 49 Grand Street, Albany, N. Y.	June 26, 1891
WOODWARD, W. C.	Electrical Engineer, Narragansett Electric Lighting Co.; residence, 21 Arlington Ave., Providence, R. I.	Nov. 18, 1896
WOODWORTH, GEO. K.	Electrician, Crawford Mfg. Co., Hagerstown, Md.	Feb. 17, 1897
WOOLF, ALBERT E.	Electrician and Inventor, Woolf Electric Disinfecting Co. of N. Y., 66 Broad St., New York City.	Sept. 16, 1890
WORSWICK, A. E.	Electrical Engineer, London and Foreign Tramways Syndicate (Ltd.) Port Elizabeth, So. Africa.	Sept. 20, 1893
WRAY, J. GLEN	Assistant Engineer, Chicago Telephone Co., 162 Centre St., Chicago, Ill.	Sept. 20, 1893
WRIGHT, LOUIS S.	General Manager, Schuylkill Electric Railway Co., Pottsville, Pa.	Nov. 18, 1896
WYBRO, HARRISON C.	Electrical Engineer, Wybro & Lawrence, 522 So. Broadway, Los Angeles, Cal.	Dec. 18, 1895
YARNALL, V. H.	Superintendent of Construction, for L. W. Serrell, 99 Cedar St., New York City.	May 16, 1893
YOUNG, CHARLES I.	Electrical Engineer, Westinghouse Elec. & Mfg. Co., Girard Building, Philadelphia, Pa.	June 27, 1895
YSLAS, CARLOS	Electrician of Railway in Jalapa, Vera Cruz, Mexico.	Nov. 18, 1896

Name.	Address.	Date of Election.
ZALINSKI, EDMUND L.	Captain of Artillery, U. S. A., (retired), The Century, 7 West 43d St., New York City.	May 17, 1887
ZIMMERMAN, LAURENCE J.	Electrical Engineer and Inventor, 57 Pennsylvania Ave., Brooklyn, N. Y.	Mar. 21, 1893

Associate Members, - - - 716.

OFFICIAL STENOGRAPHER

RYAN, RICHARD W., Room 178, Post Office Building, Telephone, 2787 Cortlandt, New York City.

SUMMARY.

Honorary Members, - - - - - - - - - -	2
Members, - - - - - - - - - - - -	351
Associate Members, - - - - - - - - -	716
Total - - -	1069